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Moment of Inertia Experiment

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This paper describes a laboratory experiment for determining moment of inertia in which the emphasis is on (1) a relatively high degree of accuracy, (2) useful application of several of the important principles of mechanics, and (3) improved instrumentation to attain the desired accuracy and still give time in the laboratory period for the student to explore the details and implications of the experiment.

WHILE extreme accuracy is not necessarily a primary aim in a general physics laboratory, several of the experiments in each term's work should be of such accuracy as to indicate that the physicist is constantly striving by refined apparatus and techniques to get more accurate results in all his work. The experiment discussed here is designed not only to teach the principles involved, but also to give a high degree of accuracy for experimental values of moment of inertia.

APPARATUS

The apparatus consists of a disk mounted on bearings so that it is free to rotate about a horizontal axis. A weight is attached to one end of a fish-line cord which is wound on the periphery of the disk, the other end of the cord being looped over a pin in the rim of the disk. This portion of the apparatus is sketched in Fig. 1.

The timing device is shown schematically in Fig. 2. The electromagnetic counter, C, is capable of recording 120 pulses per second when energized by a 60-cycle/sec source. This counter is connected in series with two single-pole single-throw switches to an alternating current source of controlled frequency. To measure the time of descent of the weight, the floor switch is initially closed and the disk switch open. When a relay at the disk is energized, the armature simultaneously releases the disk and closes the counter switch at the disk. As the weight drops, it strikes the floor switch, turning the counter off. The time of descent is accurately read on the counter.

CALCULATIONS—METHOD I

The moment of inertia, I, of a body may be determined experimentally from the ratio of the applied torque, L, to the resulting angular acceleration, α, that is, by the familiar equation,

$$I = L/\alpha. \tag{1}$$

The angular acceleration may be found by measuring the time, t, required for the mass to fall through a measured distance, s, and applying the equations of uniformly accelerated motion. Therefore,

$$\alpha = a/R = 2s/Rt^2, \tag{2}$$

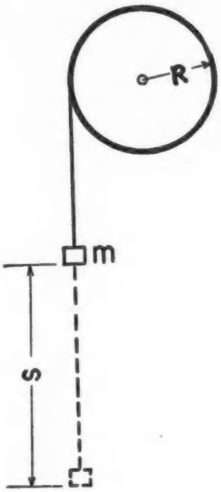

FIG. 1. Disk of radius R subject to torque produced by weight of mass m.

where R is the radius of the disk. The applied torque is given by

$$L = m(g - \alpha R)R, \tag{3}$$

where m is the mass of the falling weight, and g the acceleration due to gravity.

Substitution from Eqs. (2) and (3) into Eq. (1) will give the required results. While frictional forces are not considered, Table I shows results satisfactory for certain laboratory requirements.

CALCULATIONS—METHOD II

A correction for friction may be made by calculating the magnitude of the opposing fric-

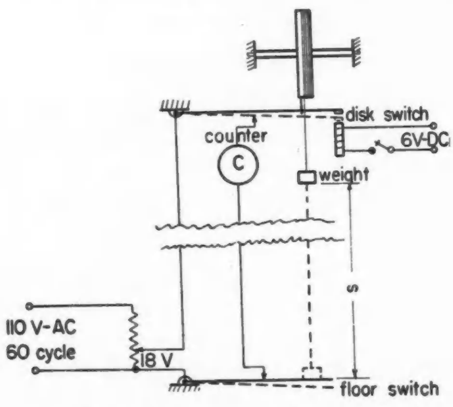

FIG. 2. Diagram of timing device applied to the mechanical system.

TABLE I. Results of calculations of moment of inertia of disk using Method I.

	Group I	Group II	Group III	Group IV
L_f (dyne cm)—Average	5,960	12,200	22,200	38,100
I (g cm^2)—Experimental	175,400	177,700	179,400	188,200
Deviation from mean (percent)	0.59	0.78	0.98	0.72
I (g cm^2)—True value	172,800	172,800	172,800	172,800
Error (percent)	1.5	2.8	3.8	8.9

tional torque, using work and energy considerations, and substituting in the equation,

$$I = (L - L_f)/\alpha. \tag{4}$$

Here L is calculated by means of Eq. (3); α by Eq. (2); and L_f, the opposing torque due to friction, as follows: The energy of the system, all in the form of potential energy before motion starts is eventually dissipated in two ways; (1) by the separation of the weight from the system as the weight strikes the floor, and (2) by friction. Therefore,

$$mgs = \tfrac{1}{2}mv^2 + L_f\theta, \tag{5}$$

where θ is the total angle in radians turned through by the disk from the start of the motion until the disk stops. This angle is easily found by counting the number of revolutions, n. Solving for L_f, we have

$$L_f = (mgs - \tfrac{1}{2}mv^2)/2\pi n. \tag{6}$$

RESULTS

The results of several groups of trials are shown in Tables I and II. Group I consists of five consecutive trials with a minimum amount of friction. Groups II, III, and IV represent trials with increasing friction applied by a simple prony brake consisting of a copper wire looped over the rotating hub of the disk. The true value of moment of inertia of the disk is taken to be that computed from the carefully measured mass and radius of the disk.

The source of 60-cycle/sec alternating current used for the data presented here was the

TABLE II. Results of calculations of moment of inertia of disk using Method II.

	Group I	Group II	Group III	Group IV
L_f (dyne cm)—Average	5,960	12,200	22,200	38,100
I (g cm^2)—Experimental	173,400	173,100	171,100	174,800
Deviation from mean (percent)	0.33	0.6	1.04	1.2
I (g cm^2)—True value	172,800	172,800	172,800	172,800
Error (percent)	0.28	0.17	0.98	1.1

commercial light and power line. If the commercial source is not sufficiently constant, a crystal-controlled oscillator with suitable frequency dividers could be used. While greater accuracy may be obtained by such further refinements, it is believed that the accuracy attained in this series of experiments is sufficient for undergraduate laboratories.

CONCLUSION

As expected, the determinations of moment of inertia with no correction for friction showed the greater error. Calculations using Method II gave consistent and reproducible values for moment of inertia, even with sizable frictional forces present.

This laboratory experiment brings to the student the application of several basic principles, including the relation between linear and rotational motion, the equations of accelerated motion, Newton's second law as applied to linear and rotational motion, and, of course, the definition and physical meaning of moment of inertia. Calculations by Method II have the additional advantage in that they bring into practical use the important concepts of work and energy.

The experiment may be easily adapted to demonstrate to the student the development of precise measurements. If the time of descent is first measured by a stop watch and then by the electric timer, the results of a series of calculations by Method I will show the advantage of refined apparatus. If, with the more accurate time found by the electric timer, the moment of inertia is calculated by both methods, the use of correction factors to secure greater precision is clearly demonstrated.

Acknowledgment is made to Dr. W. B. Pietenpol, under whose supervision this work was done, and to Mr. George L. Martin, Jr., for aid in setting up the apparatus and making the tests.

The Semidiurnal Tidal Oscillation of the Earth's Atmosphere*

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The semidiurnal barometric oscillation, with maxima at about 10 A.M. and 10 P.M. local solar time, is interpreted according to current theory as an air-tide. Although the lunar tidal force is 2.2 times more powerful than the solar, and hence lunar tides in the oceans are 2.2 times stronger than the solar, this is not found to be the case in the atmosphere. The observations show a solar semidiurnal atmospheric oscillation about 100 times greater than might be expected, and a very feeble lunar oscillation. The difficulty is resolved with a suggestion by Kelvin. This is the famous "resonance theory," and illustrates how the effect of a comparatively small tide-generating force might be magnified, if the atmosphere had a period of free oscillation close to 12 solar hours. The linearized equations of atmospheric oscillations are stated, tidal wind fields indicated, and conclusions summarized.

THE gravitational forces exerted by the moon and sun are, as we all know, the cause of tides in the oceans. The tide-raising force is proportional to M/d^3, where M is the mass of the disturbing body, and d its distance from the center of the earth. The moon, by virtue of its proximity to the earth exerts, in spite of its smaller mass, a tidal force 2.2 times more powerful than the sun. As we might expect, the lunar tide dominates the solar tide in the oceans in this ratio of 11:5. The earth's atmosphere also is under the influence of tidal forces. It would seem that here too the lunar tide should dominate in the same ratio. This, however, is not found to be the case.

What is actually observed, by attention to the

* Based on a paper presented at the Twentieth Annual Meeting of the American Association of Physics Teachers at Barnard College and Columbia University, New York City, February 1, 1951. See H. L. Stolov, Am. J. Phys. **19**, 329 (1951).

† The general problem of atmospheric oscillations is being studied at New York University under the direction of Professor Bernhard Haurwitz through support and sponsorship extended by the Geophysical Research Directorate, Air Force Cambridge Research Laboratories under Contract No. AF 19(22)-49.

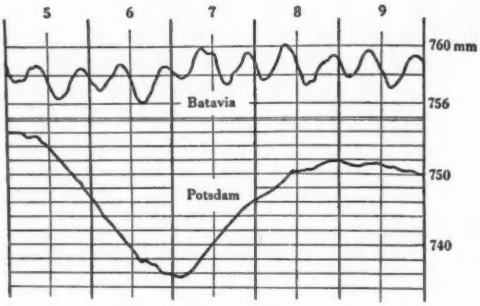

FIG. 1. A barograph trace taken in the tropics and one taken in the middle latitudes are compared.

fluctuations of the barometer at sea level, is a solar semidiurnal tidal oscillation about 100 times greater than the equilibrium value, and a lunar semidiurnal tide so feeble at middle latitudes that it escaped detection by Laplace (1823) and Airy (1877), only to be exposed by Chapman (1918).[1] The amplitude of the semidiurnal lunar tide at Greenwich was found to be 0.009 mm of mercury. This is less than the error of reading the photographic barograph from which the data were obtained. The fact that a periodic term of such small magnitude can be determined, is a remarkable result of the combination of very many readings taken during those days of daily barometric range less than 0.1 inch and hence of reduced random variation.

Before reviewing some suggested explanations for the tidal paradox in the atmosphere, let us examine the periodic barometric fluctuations, for they are the most conclusive evidence of the existence of tides in the atmosphere.

Since the end of the seventeenth century, at which time the barometer of Torricelli began to make its way into the tropics, it was observed that the height of the barometer in the tropics remained quite constant, as compared with the barometer in the middle-latitudes. Superimposed upon this unchanging state, there seemed to be a small semidiurnal fluctuation in height, which could be detected by plotting the hourly height values for several days. A barograph trace is reproduced in Fig. 1 for Batavia, 6°16′S, and Potsdam, 52°25′N, by way of comparison. We see at the tropical station the semidiurnal pressure variation with maxima at 10 A.M. and 10 P.M. and minima at 4 A.M. and 4 P.M. local solar time. In fact, the remarkable regularity of this semidiurnal pressure wave is such that, if one's wrist watch were out of order in the tropics, a barometer might serve as a timepiece with some degree of accuracy.

We interpret this tropical oscillation of the barometer as an air-tide. In middle latitudes, we expect to find a similar effect operating, but masked by the large scale motions of tropospheric weather systems and their accompanying irregular pressure changes. Bennett[2] shows, by taking hourly averages of barometer readings for 14 years at Washington, D. C., an average daily pressure variation that is largely semidiurnal

TABLE I. Amplitude (s_n) and phase (S_n) of 24-, 12-, 8-, 6-hourly components of the pressure variation.

Station	Latitude	s_1	S_1	s_2	S_2	s_3	S_3	s_4	S_4
Batavia	6.2S	537	17.0	998	156.9	10	203.8	32	60
Dar-es-Salaam	6.8S	787	−16.3	790	153.7	34	145.4	12	104
Bombay	18.9N	518	−27.6	1090	158.5	168	357.5	69	−120
Calcutta	22.5N	737	−29.6	1043	152.6	193	356.8	63	−135
São Paulo	23.6S	345	3.5	631	152.2	84	183.4	9	35
Curityba	25.4S	406	16.3	726	145.0	115	191.5	21	87
Johannesburg	26.2S	484	− 7.7	594	145.6	108	169.3	27	20
Kimberley	28.7S	808	− 2.3	605	155.2	91	180.9	...	...
Cordoba	31.4S	832	2.6	668	141.5	174	161.3	...	...
Rosario	32.1S	857	− 1.4	553	161.0	134	178.3	...	...
Montevideo	34.9S	436	−24.5	367	144.5	110	142.0	15	8
Melbourne	37.8S	338	12.3	577	161.4	124	175.1	...	...
Lisbon	38.7N	80	−21.2	461	154.5	182	353.2	...	...
Milan	45.5N	141	9.8	298	149.7	125	354.5	...	...
Geneva	46.2N	110	−19.5	350	166.5	90	3.8	...	...
Kremsmünster	48.1N	131	13.4	268	151.2	108	8.9	...	...
Munich	48.2N	86	−12.7	169	160.9	122	357.3	...	...
Prague	50.1N	178	3.2	196	140.8	106	354.1	...	...

[1] Chapman, Quart. J. Roy. Meteorol. Soc. **44**, 271–280 (1918).
[2] Bennett, Monthly Weather Rev. **34**, 528–530 (1906).

in character. The pressure variation at any station, subjected to harmonic analysis, can be well represented by the first four terms of the series

$$p=\sum_{n=1}^{4} s_n \sin(nt+S_n), \tag{1}$$

where t is local solar time in degrees measured from midnight to midnight, one hour equal to 15° and s_n is the amplitude of the n^{th} vibration of period $360/15n$ hours and phase S_n. Hence the total pressure variation may be regarded as made up of four sinusoidal pressure waves $n=1$, 2, 3, 4 to which correspond the periods 24, 12, 8, 6 hours, respectively. Table I shows the amplitude and phase of the four pressure waves for various stations during the month of January. Amplitudes are given in units of 0.001 mm and phase angles are in degrees. The table represents a selection by Wilkes[3] from extensive data published by Hann (1889, 1892, 1918), and Pramanik (1926).

Examination of the extensive data of Hann and Pramanik, of which Table I is a small but representative sample, allows us to make certain statements. The 8- and 6-hourly components are very small and very irregular and show marked seasonal, annual, and local variations. They are of doubtful physical reality. The 24-hourly component, although only slightly smaller than the 12-hourly component, is not of universal character. It shows considerable variation in amplitude and phase as the station is located in level or mountainous, maritime or continental regions, and is undoubtedly the result of a subtle combination of geography, diurnal tidal component, and local thermal action. The most important oscillation is the 12-hourly component, the semidiurnal tidal oscillation of the earth's atmosphere. The regularity, universality, and constancy of phase, the uniform decrease of amplitude with latitude, are all evidence of its physical reality.

If we confine our attention to the semidiurnal oscillation, we find that the times of maxima and minima for low latitude stations occur at the same local time (Table II), whereas for high latitude stations they occur at the same Greenwich time (Table III).

TABLE II. Time of maximum pressure for low latitude stations (Simpson).

Station	Latitude	Longitude	Local time of max. pressure A.M. and P.M.
Quito	0°14′S	78°32′W	10.0
Para	1°27′S	48°29′W	10.0
Quixeramdbin	5°16′S	39°56′W	10.3
Ascension	7°55′S	14°25′W	9.8
Gabun	0°25′N	9°35′E	9.9
Kwai	4°45′S	38°18′E	10.0
Zanzibar	6°10′S	39°10′E	9.9
Dar-es-Salaam	6°49′S	39°19′E	9.6
Singapore	1°15′N	103°51′E	9.8
Batavia	6°11′S	106°50′E	9.7

The data shown in Table II and Table III are selections from more extensive data of Simpson.[4] They suggest that the semidiurnal wave could be represented by the sum of two sinusoidal components, one depending on local time and the other on Greenwich time. We may, therefore, write

$$s_2 \sin(2t+S_2)=b\sin(2t+B) + c\sin(2[t-\phi]+C), \tag{2}$$

where ϕ is longitude east of Greenwich, $(t-\phi)$ Greenwich time, and b, B, c, C, are functions of latitude. If we had observations of s_2 and S_2 at four points along the same latitude circle, we would have four equations in four unknowns. If we had similar observations along other latitude circles, we could write the general expression. Lacking this, but with data for the amplitude and phase of the semidiurnal barometric oscillation at 190 stations over the earth, Simp-

TABLE III. Time of maximum pressure for high latitude stations (Simpson).

Station	Latitude	Longitude	Local time A.M. and P.M.	Greenwich time A.M. and P.M.
Point Barrow	71°17′N	156°40′W	0.7	11.1
Polarishaus	78°18′N	72°51′W	6.2	11.0
Fort Conger	81°44′N	64°45′W	7.2	11.5
Polaris Bay	81°36′N	62°15′W	7.4	11.5
Sabine Island	74°32′N	18°49′W	10.0	11.3
Jan Mayen	70°59′N	8°28′W	10.4	11.0
Cap Thorsden	78°28′N	15°42′E	11.7	10.6
Polhem	79°50′N	16° 4′E	0.3	11.2
Nova Sembla	72°23′N	52°43′E	2.4	10.8
Ssagastir	73°23′N	124° 5′E	7.5	11.2

[3] Wilkes, *Oscillations of the Earth's Atmosphere* (Cambridge University Press, London, 1949), esp. p. 8.

[4] Simpson, Quart. J. Roy. Meteorol. Soc. **44**, 1–18 (1918).

TABLE IV. Amplitude and phase of the equatorial and polar components of the semidiurnal oscillation (Simpson).

Mean latitude	Equatorial vibration b (mm)	B	Polar vibration c (mm)	C
0°	0.920	156°50′	0.068	− 3°58′
18°N	0.835	155°17′	0.082	−23°13′
30°N	0.628	149°7′	0.059	10°26′
40°N	0.387	153°56′	0.043	91°4′
50°N	0.240	153°1′	0.041	104°27′
60°N	0.096	158°3′	0.062	108°23′
70°N	0.022	152°53′	0.072	98°34′
80°N			0.080	116°27′

son grouped his data into eight zones, reduced to sea level, and corrected it by an approximate formula to the mean latitude of the zone. The observations in each zone were treated independently, and the values for b, B, c, C, determined by the method of least squares. The results are given in Table IV.

The expression forwarded by Simpson for the semidiurnal pressure oscillation based on Table IV is

$$s_2 \sin(2t+S_2)=0.937 \sin^3\theta \sin(2t+154°) +0.137(\cos^2\theta-\tfrac{1}{3})\sin(2[t-\phi]+105°), \quad (3)$$

where θ is the colatitude. The first term on the right side of Eq. (3) represents, in the terminology of Simpson, the equatorial vibration, since it is a maximum at the equator. It might also be labeled the progressive wave, since it describes a pressure wave traveling parallel to the latitude circles from east to west round the earth with the sun. The second term on the right-hand side of Eq. (3) represents, in the terminology of Simpson, the polar vibration, since at high latitudes, where the progressive vibration is small it is the dominant influence. It might be labeled the stationary wave, since it describes a meridional oscillation of air with pressure node ($\cos^2\theta=\frac{1}{3}$) at 35°16′N and S latitude. Simpson's treatment of the polar component has recently been criticized by Wilkes,[5] who offers an alternative expression. The purely empirical formula of Wilkes, although a better fit to the data below 50° latitude, where the error in determination of the stationary wave may be considerable, is less accurate above 50° latitude, where the computational errors for the polar vibration would be smaller. This stationary

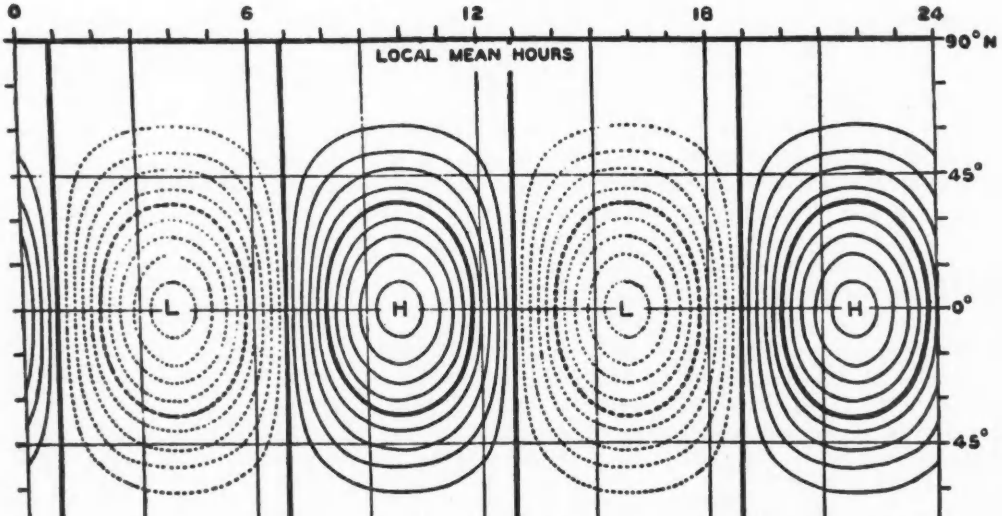

FIG. 2. The east-to-west progressive wave of the semidiurnal pressure oscillation illustrated in cylindrical projection. Isobars are for intervals of 0.1 mm of mercury, and longitudes are indicated by the local time (Bartels).

[5] See reference 3, pp. 14–15.

wave makes only a minimum contribution to the semidiurnal oscillation at all but high latitude stations. Its origin is obscure. The progressive wave, on the other hand, makes the maximum contribution and will be our primary concern.

In order to understand more clearly the physical picture, we might compare the oscillations of the atmosphere with the oscillations of a self-gravitating globe of liquid when released from a constrained spheroidal configuration, oscillating about the spherical form, alternately prolate and oblate. The maximum of pressure for the semidiurnal oscillation occurs, as we have seen, at 10 A.M. and 10 P.M., yet, according to tidal theory, it ought to occur after the transit of the heavenly body, or for a solar tide, after 12 noon. There are undoubtedly thermal influences as well as tidal influences of the sun that combine to produce this phase acceleration.[6] The atmospheric particles that propagate the east to west pressure wave do not, of course, travel round the earth, but oscillate nearly horizontally with a range of about 4 miles at the surface.

The progressive component of the semidiurnal oscillation,

$$p=0.937 \sin^3\theta \sin(2[t'+\phi]+154°), \qquad (4)$$

where t' is Greenwich time, describes the world-wide pressure field illustrated in cylindrical projection in Fig. 2. Associated with this pressure system, there is a world-wide wind system that can be computed.

The progressive wave will be treated as a small perturbation upon an undisturbed state. Hence the total pressure $p'=p_0+p$, where p_0 is the static pressure near the ground. The horizontal equations for small motions, neglecting quadratic terms in the perturbation quantities, on a spherical rotating earth are

$$\partial u/\partial t-2\omega v \cos\theta = -(1/\rho_0 a)(\partial p/\partial\theta), \qquad (5)$$

and

$$\partial v/\partial t+2\omega u \cos\theta = -(1/\rho_0 a \sin\theta)(\partial p/\partial\phi), \qquad (6)$$

where ρ_0 is the average air density near the ground, u the velocity component toward the south, v the velocity component toward the east, ω the angular velocity of the earth's rotation, a

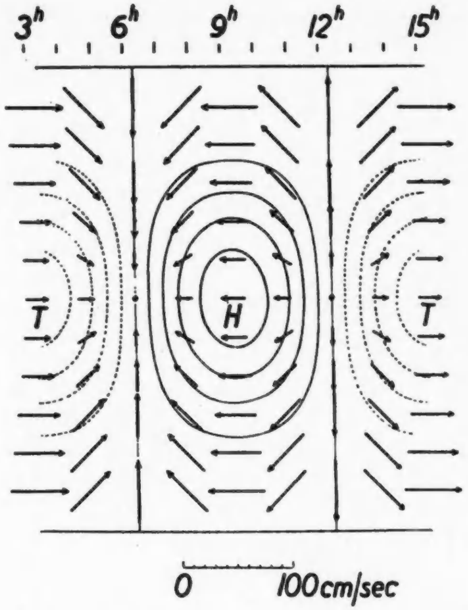

FIG. 3. The world-wide wind system at the surface produced by the progressive pressure wave of Fig. 2. Isobars are for intervals of 0.2 mm of mercury, and wind arrows are to scale (Bartels).

the radius of the earth, θ the colatitude, and ϕ the longitude. Substituting for the progressive wave, $p=A \sin(2[\omega t+\phi]+154°)$, we have upon solving Eqs. (5) and (6) for the velocity components[7]

$$u=\frac{f+g\cos\theta}{2\sin^2\theta}\cos(2[\omega t+\phi]+154°), \qquad (7)$$

and

$$v=-\frac{g+f\cos\theta}{2\sin^2\theta}\sin(2[\omega t+\phi]+154°), \qquad (8)$$

where

$$f=\frac{RT_0}{p_0 a\omega}\frac{\partial A}{\partial\theta}, \qquad (9)$$

$$g=\frac{2RT_0 A}{p_0 a\omega \sin\theta}, \qquad (10)$$

and

$$p_0=\rho_0 RT_0. \qquad (11)$$

The tidal winds at the surface, computed from Eqs. (7) and (8) are of the order of 1 mile

[6] Chapman, Quart. J. Roy. Meteorol. Soc. 50, 165–195 (1924).

[7] Mitra, *The Upper Atmosphere* (Royal Asiatic Society of Bengal, Calcutta, 1948), Chapter II, esp. p. 34.

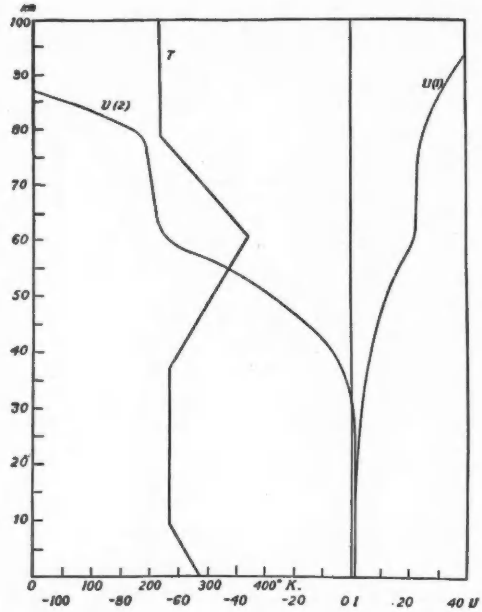

FIG. 4. The idealized temperature distribution in the atmosphere, assumed by Pekeris, is shown by curve T. The ratio of the horizontal velocity at any level to the value at the ground is illustrated for the two modes 10.5 hours and 12 hours by the curves $U(1)$ and $U(2)$, respectively.

per hour, and are depicted in Fig. 3. The experimental observation of such winds on the surface would be difficult to accomplish because of considerably stronger tropospheric wind systems.

Now that we have discussed the barometric evidence of atmospheric tides and the consequences of such pressure oscillations, let us return to the possible explanations of the tidal paradox (strong solar tide and weak lunar tide in the atmosphere). Historically, the first explanation was offered by Laplace,[8] who suggested that the barometric oscillation is not of tidal but of thermal origin. Kelvin[9] accepted the thermal hypothesis, but pointed out that since the diurnal variation of temperature is much larger than the semidiurnal, it is strange that the semidiurnal variation of pressure should be greater. The one sentence of Kelvin from which the resonance theory of atmospheric tides originates is quoted.

> When thermal influence is substituted for gravitational, in the tide-generating force reckoned for, and when the modes of oscillation corresponding respectively to the diurnal and semi-diurnal terms of the thermal influence are investigated, it will probably be found that the period of free oscillation of the former agrees much less nearly with 24 hours than does that of the latter with 12 hours; and that, therefore, with comparatively small magnitude of the tide-generating force, the resulting tide is greater in the semi-diurnal term than in the diurnal.

The difference between the lunar and solar semidiurnal periods is approximately 26 minutes. Hence, it is conceivable, if the natural period of vibration of the atmosphere were close to 12 solar hours, that the solar influence would be magnified and rendered much more effective than the lunar. In fact, a hundredfold magnification of the solar tide is indicated theoretically for a free period within 6 minutes of 12 hours. What remains is to show that the atmosphere has such a period of free vibration.

Following along the lines of Laplace (isothermal atmosphere with isothermal changes of state), Margules[10] showed that the atmosphere has a free period of 11.94 hours and no free period near 24 hours. Since we are dealing with long wavelengths, on the order of the earth's diameter, it is unrealistic to suppose that conduction, convection, and radiation would accomplish a complete equalization of temperature differences during the available time interval. A more tenable assumption would be adiabatic changes of state in an atmosphere in convective equilibrium (a temperature decrease linear with height at the adiabatic lapse rate or about 10°C per km). This was considered by Lamb,[11] but on a plane earth. Haurwitz[12] developed the theory further for the general case of the oscillations of an autobarotropic atmosphere on a spherical rotating earth. This assumption implies that the same law holds for the distribution in space of the density as function of the pressure and for the change of the density of an individual particle with the pressure. The isothermal atmosphere of Laplace and Margules, and the adiabatic atmosphere of Lamb are then special cases of this more general treatment. It was shown by

[8] Laplace, *Mécanique Céleste* (Paris, 1823), Livre 4, Chapitre 5.
[9] Kelvin, Proc. Roy. Soc. (Edinburgh) 11, 396 (1882).

[10] Margules, Sitzber. Akad. Wiss. Wien, abt. IIa, **101**, 597–626 (1892) and **102**, 11–56 (1893).
[11] Lamb, Proc. Roy. Soc. (London) **A84**, 551–572 (1910).
[12] Haurwitz, Gerlands Beitr. Geophys. **51**, 195–233 (1937).

Taylor[13] for an atmosphere on a rotating spherical earth, where the temperature is an arbitrary function of height, that free oscillations are possible, except that the amplitude of the oscillation varies with height. Pekeris[14] continued the work of Taylor, by assuming a temperature distribution with height idealized from the actual distribution that we believe to exist in the first 80 km (curve T of Fig. 4). This atmosphere is shown to have two important modes of free vibration of period 10.5 hours and 12 hours with a variation of amplitude with height $U(1)$ and $U(2)$, respectively (Fig. 4).

The mathematical theory of atmospheric oscillations excited by gravitational action is quite complex (thermal action would be a further complication), and still incomplete at this time. It consists in solving the Eulerian hydrodynamic equations on a rotating spherical earth. The oscillation is treated as a perturbation upon a state of rest, and the equations are linearized. Quadratic terms in the perturbation quantities are neglected as small quantities of higher order, and the vertical acceleration is assumed to be negligible as is the variation of g with height.

The complete mathematical development will not be reproduced here.[15] The fundamental equations will be stated and the conclusions summarized.

If we take u, v, w the south, east, and vertical components of the velocity, ω the angular velocity of the earth, a the radius of the earth, θ the colatitude, ϕ the longitude, g the acceleration of gravity, Ω the tidal potential, c the local velocity of sound, p' the total pressure ($p' = p_0 + p$), p_0 the static pressure, p the pressure variation due to the oscillation, ρ' the total density ($\rho' = \rho_0 + \rho$), ρ_0 the static density, and ρ the density variation due to the oscillation, the equations of motion are

$$\partial u/\partial t - 2\omega v \cos\theta = -(1/a)(\partial/\partial\theta)(p/\rho_0 + \Omega), \quad (12)$$

$$\partial v/\partial t + 2\omega u \cos\theta = -(1/a \sin\theta)(\partial/\partial\phi)(p/\rho_0 + \Omega), \quad (13)$$

$$\partial p/\partial z = -g\rho - \rho_0(\partial\Omega/\partial z), \quad (14)$$

the equation of continuity is

$$(\partial\rho/\partial t) + w(\partial\rho_0/\partial z) + \rho_0\chi = 0, \quad (15)$$

where χ = divergence of velocity,

$$\chi = (1/a \sin\theta)\partial(u \sin\theta)/\partial\theta + (1/a \sin\theta)(\partial v/\partial\phi) + (\partial w/\partial z), \quad (16)$$

and the adiabatic equation of state is

$$(Dp'/Dt) = c^2(D\rho'/Dt). \quad (17)$$

By the elimination of u, v, w, p and ρ, a second order differential equation for the divergence is obtained, which may be solved by the method of separation of variables. The nature of the boundary condition to be imposed at low levels is simply that the vertical velocity must vanish. At high levels, the vertical flow of energy, if not zero, must be in an upward direction. The latter condition is understandable, when we consider that the tidal energy is introduced mostly at low levels where the air density is greatest.

Some results of the Taylor-Pekeris theory of atmospheric oscillations are summarized.

1. The atmosphere has a free period of 12 hours required by resonance theory, and a free period of 10.5 hours deduced as the free period excited by the Krakatoa eruption and the Great Siberian meteor.

2. There is general agreement between the empirical formula of Simpson discussed earlier and the theoretical calculations.

3. For the resonant mode, the ratio of the horizontal velocities at any level to the value at the ground is shown by curve $U(2)$ of Fig. 4. There is a nodal surface at about 30 km, where the horizontal velocities and pressure oscillations vanish. The atmospheric layers above and below this level move mainly horizontally, in opposite directions with pressure changes of opposite sign. The velocities increase rapidly with height and at 100 km are more than 100 times their ground value of 1 mile per hour.

4. The diurnal variation of the earth's magnetic field, explained by the dynamo theory as the result of the horizontal movement of the conducting layer across the earth's magnetic field, requires that the pressure oscillation in the layer be 180° out of phase with the observed pressure oscillation at the ground, and further

[13] Taylor, Proc. Roy. Soc. (London) **A156**, 318–326 (1936).
[14] Pekeris, Proc. Roy. Soc. (London) **A158**, 650–671 (1937).
[15] For a complete discussion see Wilkes (see reference 3) or Pekeris (see reference 14).

requires that the conductivity of the layer be much greater than that previously deduced from ionospheric data. Both difficulties are resolved by the resonance theory, since the required phase shift is shown to exist between low and high levels, and the velocity of the particles in the conducting layer is shown to be great.

The prediction that the middle and high atmosphere constitute a region swept by an intense, periodic, world-wide tidal wind system with velocities exceeding 100 miles per hour, is not without experimental verification. Störmer reports, in the publications of the University Observatory at Oslo 1932–1934, measured wind velocities of about 135 miles per hour for noctilucent clouds at 80 km. The study by Olivier of long enduring meteor trails at elevations of 80–90 km results in wind velocity measurements of approximately the same magnitude. A recent important study of winds in the ionosphere by radio methods performed by Mitra[16] reports a most frequent wind velocity of 110 miles per hour at or below the sporadic E region (70–115 km). The measurements clearly indicate that the wind velocity varies with a semidiurnal period. Although there is general agreement between the experimental results of Mitra and tidal theory as regards the magnitude of the wind, there does not appear to be close agreement for the direction. We are told that additional experiments are being performed.

In conclusion, we may say that much work remains to be done before the resonance theory of atmospheric oscillations can be regarded as complete and incontestable. Experimentally, we will want to acquire more knowledge of the physical state and properties of the upper atmosphere. Theoretically, we will want to improve our over simplified equations by perhaps taking into account hitherto neglected factors such as the nonlinear terms, the zonal motion of the atmosphere in the equilibrium state, the effect of temperature increase with height, solar thermal action, damping forces, and the high level effects of charged particles moving in the earth's magnetic field.

The author expresses his thanks to Professor Bernhard Haurwitz, Chairman of the Department of Meteorology at New York University, for his helpful comments and suggestions on the manuscript, and to Professor Mark W. Zemansky, for the encouragement that led to the original presentation of this paper before the American Association of Physics Teachers.

[16] Mitra, Proc. Inst. Elec. Engrs., Part 3 **96**, 441–446 (1949).

But the position may become a dangerous one on both sides, for the layman may come to look on the scientist as the priest of an esoteric religion which he respects but does not understand, or perhaps even want to understand, and again the scientist is sometimes also a little inclined to claim the attributes of the priest, and regard himself as the master of mysteries which will forever be beyond the ken of the flock. Apart from the fact that this makes him a very insufferable person, it has the dangers of isolation. There are many cases in history of scientific schools which have worked with extraordinary energy and intelligence on subjects which seem entirely trivial to us now. We must recognise that almost any subject in the world if studied deeply enough becomes interesting, and that this exposes us all to the insidious dangers of pedantry, for the pedant is only the learned man whose mind is engaged with unimportant things. Therefore the scientist should be ready sometimes to justify the importance, I certainly do not wish to say the utility, of his studies before the general tribunal.—C. G. Darwin, THE NEW CONCEPTIONS OF MATTER, 1931.

John Wesley Hornbeck*

Recipient of the 1950 Oersted Medal for Notable Contributions to the Teaching of Physics

The American Association of Physics Teachers has made to Professor John Wesley Hornbeck, lately Professor of Physics at Kalamazoo College, the fifteenth of its annual awards for notable contributions to the teaching of physics. Following an address of recommendation by Professor J. W. Buchta, Chairman of the Committee on Awards, the presentation was made by Professor Duane E. Roller, President of the Association, in a ceremony held in McMillan Hall, Columbia University, on February 2, 1951, during the twentieth annual meeting.

Address of Recommendation by Professor J. W. Buchta, Chairman of the Committee on Awards

THE Committee on Awards has reviewed the recommendations submitted to it, and, on the basis of a preferential ballot, approved by the executive committee, designates PROFESSOR JOHN WESLEY HORNBECK as the recipient of the 1950 Oersted Medal.

Dr. Hornbeck has done his teaching at Carleton College in Minnesota from 1913 to 1925 and at Kalamazoo College in Michigan since 1925. This year he retires from active teaching duties.

The college teacher may be said to be one who does not follow his profession to live but who lives to follow his profession. The college teacher is to science and the community of scientists as a mother is to the family she raises. His contributions cannot be measured; they are seldom acclaimed or honored.

But the American Association of Physics Teachers has annually, for a number of years, honored an outstanding teacher. Dr. Hornbeck will join a very respectable company.

The basis of selection is not rigidly set. In making the present award, the committee received and considered many statements and appraisals from former students and colleagues. It is, after all, they who do the selecting—they who have sat in his classes and remember the spontaneity of his remarks and the breadth of his guidance in their educational ventures. They now write about the visits to his home, the discussions and conference sessions about the dining room table.

Dr. Hornbeck's interests are not restricted to physics. Birds, their habits and characteristics, make up one hobby; a bird arriving early in the spring or a strange one in the woods may receive hours of attention with field glasses. His teaching has included mathematics and astronomy. Indeed, he first met the girl who was to become Mrs. Hornbeck in a high school mathematics class of his before he began college teaching. I know he taught astronomy because a student in another class remembers that, in showing some slides, including a photograph of Mars, Dr. Hornbeck ruefully apologized for having no decent picture of Venus.

But the very substantial character of his teaching is proven by the product of his classroom—by the number of students who have gone on to graduate work. Many of them attribute their decision to the influence of Dr. Hornbeck. I may add that his own son presented an invited paper before the American Physical Society during these present joint meetings.

It has been recognized by a number of graduate departments that they can rely on the recommendations and appraisals of Doctor Hornbeck. They are valid; he recognizes ability when he encounters it in the youth; he fosters and develops it, and then sponsors the young scientist in his endeavors to go beyond the bachelor's degree. Dr. Hornbeck can and does receive as much satisfaction in seeing a student launched on a successful research or teaching career as may another physicist in discovering a new isotope.

A week ago Dr. Hornbeck was in a hospital. We are extremely glad that he can be with us today. President Roller, may I present John Wesley Hornbeck as the Oersted Medalist for 1950?

* See Am. J. Phys. 19, 324 (1951).

Some Reflections on the Teaching of Physics

JOHN W. HORNBECK*
Kalamazoo College, Kalamazoo, Michigan
(Received January 12, 1951)

Response of the recipient of the Oersted Medal of the American Association of Physics Teachers at the ceremony of presentation. The teaching of physics in a small liberal arts college is discussed, and the values of a liberal education are stressed.

THE presentation of the Oersted Medal to a physicist from a small liberal arts college is a recognition that these institutions as a group, as well as the large universities, are continuing to make important contributions to physics and physics teaching. It is appropriate, therefore, that my remarks this afternoon concern physics and the teaching of physics in a college of liberal arts.

In order to understand or appreciate what I have to say, a few words are in order concerning Kalamazoo College, where I have been for the last twenty-six years. We are co-educational, and we are a small college. The enrollment in recent years has been about 600. We are wholeheartedly committed to the ideal of liberal education. We grant no B.S. degrees, but only the A.B. This means exactly what it implies. Students are required to take at least one year of work in each of the four main divisions of the curriculum; all students must complete a major and two minors, one of which must be unrelated to the major; and all must take two years of foreign language. But the curriculum is not static for we have a standing committee of the faculty which studies the curriculum constantly and often recommends changes. However, the faculty and administration are unanimous in their support of the four-year course of the liberal arts type. As a private institution we do not have to offer a course in dietetics for the women, nor a course in garage mechanics for the men, no course in tinkering with airplane engines.

Kalamazoo College is situated precisely halfway between Detroit and Chicago. Such an institution in the center of the midwest and in the middle of the twentieth century will, of course, remain small. It will not attract the crowds. Out of the great onrush of youth into the large universities around us, we deflect a few delightful young people who have found a challenge in our ideal of a broad education, and who are willing to take the time for it, even though in many cases it may mean extra time spent in preprofessional training. It is well known that the professional schools and especially the graduate schools of this country depend upon the liberal arts colleges as an important source of supply of good students.

I hasten to remark that we, like most liberal arts colleges, are not sensitive about being small. We may be only one atom in the vast crystal of higher education; but we believe we are an important atom, a charged atom, our charge being the responsibility to help other similar colleges carry the torch for liberal education in America. With our small classes, often composed of a few students grouped around a table for discussion or in one of our seminar homes, our group life is congenial, and with genuine pride we refer to it as "a fellowship in learning."

You will want to know what kind of conditions and opportunities there are for teaching physics in a college like Kalamazoo College. Very briefly, here are a few pertinent facts. We have a modern, fireproof building for physics and chemistry, the gift of the late R. E. Olds, automobile manufacturer of Lansing, Michigan. The physics department occupies two floors of the Science Hall, and there is ample space for lecture rooms, offices, darkrooms, and laboratories large and small. We have a generous annual allowance in the budget for equipment, supplies, student help, and books for the library. Our staff seems to be adequate; we have three in chemistry and two in physics, and all have their Ph.D. degrees.

* The Association regrets deeply the death of Dr. Hornbeck on February 27, 1951, less than one month after the presentation of this paper. See Am. J. Phys. 19, 324 (1951).

This means that no one with a faculty rank below that of assistant professor is teaching physics or chemistry in our College. In Olds Science Hall we coordinate our courses in physics and physical chemistry; it is a genuine pleasure to state that the five of us work together as a team, with friction and professional jealousy wholly nonexistent. Whatever success has attended our work in Olds Science Hall at Kalamazoo College has been largely due to the happy combination of good team work, plus exceptional equipment, plus a succession of very able young people who have come our way.

On teaching methods and techniques I have nothing spectacular to report. Naturally, one can not help learning a great deal by experience, by trial and error, in forty years of teaching various courses in undergraduate physics. I even had a try at teaching the Signal Corps in the First World War and the Army in the second war. If we become further involved in a third war, I shall feel inclined to hibernate with the black bear in Northern Michigan. But actually in such an event I would, of course, line myself up again on the side of duty.

Through the years I have tried seriously and constantly and consciously, as most teachers do, to keep up my intellectual growth, to avoid stagnation. In some ways the teacher has more of a problem here than does the research physicist. In so far as these efforts have been successful, the reasons are not hard to discover. In the first place, I have found certain magazines and periodicals most stimulating and rewarding. First among these I rate the *American Journal of Physics;* also very helpful have been the *Reviews of Modern Physics*, *Physics Today*, and the rest of the publications of the American Institute of Physics. Two journals of a general scientific character that have proved useful are *The American Scientist* and the rejuvenated *Scientific American*. For more general information in the field of education and for trends in college teaching, I depend upon the *Journal of Higher Education*, the *Educational Record*, the *Association of American Colleges Bulletin*, and the like.

In the second place, I rate very highly the value of scientific meetings which offer recurring opportunities to talk with colleagues in other institutions and discover other points of view. In addition to the large annual meetings of the American Association of Physics Teachers I mention Professor G. W. Stewart's summer Colloquium each June in Iowa City; and our two meetings a year of the Michigan Teachers of College Physics, a unique organization because we have no officers and no dues. In the fall of each year this group meets at the University of Michigan, in the spring at some other college or university, and the host institution arranges the program.

In the third place, I can never forget the influence and inspiration of some of my former teachers. Of these, two stand out in my memory most vividly today: I refer to F. R. Watson of the University of Illinois, retired but still living, and the late revered F. K. Richtmyer of Cornell University. Both of these men helped me immeasurably when as a very young graduate student I was trying to learn to teach physics.

In the fourth place (and my points are not arranged in order of importance), one of the most powerful drives that has helped me to keep on my toes as a perpetual physics student has been my own class in modern physics. The one unchanging thing about my teaching schedule for many years has been this three-hour course throughout the year, open to students who have completed a year of general college physics. It might be classified as a Rogers block-and-gap[1] type of course. We select a number of topics and trace each of them historically from the beginning of the research or the discovery of the phenomenon down to the present time; for example, we study the work on the electronic charge and the charge-to-mass ratio e/m for electrons, the development of the mass spectrograph, photoelectricity, x-rays, excitation and ionization potentials, radioactivity, and the modern attack on the nucleus of the atom. By giving particular attention to experimental details, to the apparatus used, and to the interpretation of data in a few of the more important investigations, we gradually clarify our knowledge of the phenomenon under discussion. The work of the students is done chiefly in the library where about fifty books on various aspects of modern physics are on open reserve, and where the publications of

[1] Eric M. Rogers, Am. J. Phys. **17**, 532 (1949).

the American Institute of Physics and other scientific periodicals are available. Individual students are often assigned special topics in advance, and these are taken up in succession according to an outline. Every report on one of these topics is discussed by the whole group before the next one is presented, and I take my turn with the students in this activity. I lecture no more than once a week on the average. Incidentally, I am forced to learn each year as much as any one in the class, for my policy is never to dodge or hedge about any question that arises. We have a good time together and really make our class a fellowship in learning.

A certain feature of our arrangement of rooms for physics has proved most valuable. We have three small private laboratories designed for research on any prolonged experiment. Regularly one or two students are assigned to one of these rooms for several weeks at a time, or possibly for a whole semester, in order to work undisturbed on some laboratory project that provides good training for research. There has been a constant demand for such special work, which is recorded in the Registrar's Office as an individualized course.

Perhaps a word should be said about our work in general college physics. For several years we have had two entirely separate courses in general physics; one, a five-hour course for physics and chemistry majors and pre-engineers with trigonometry as a prerequisite; the other, a four-hour course for everybody else, including premedical and predental students, with no mathematical requirements beyond high school algebra and geometry. Oddly enough, the two classes have run about equal in size. Our line of cleavage between the two groups has seemed natural enough, and the scheme has worked well in practice, especially because either course is accepted as meeting a graduation requirement of one year of laboratory science.

This year, however, we are trying an entirely different plan. Dr. Barbour and Dr. Strong of our staff are collaborating on a four-hour course in physical science for nonscience majors, using carefully selected and correlated material from both physics and chemistry. These men will report on their experiment for themselves in due time. Suffice it here to remark that, as the first semester comes to a close, these men as well as their students are notably enthusiastic about the undertaking which is planned to be a one-year terminal course.

I can make no pronouncements this afternoon about the best way to teach physics. In fact, I do not believe there is one best method which will guarantee success. A teacher will naturally use different methods in different classes, depending on the kind of subject matter and the size of the class. However, in general college physics certain methods and techniques have seemed to me to be especially effective and important. Some of the statements that follow illustrate teaching practices that professors are able to employ only in small liberal arts colleges. Everyone recognizes that large universities and schools with other objectives than granting a Bachelor of Arts degree cannot utilize practicably some of these methods, even if they agree with their aim, which is ***individualized instruction***.

1. Laboratory classes should be kept very small and well supervised. Our sections are limited to twelve students at one time and no more than two students work together on the same experiment. A student assistant and I circulate constantly in order to answer and ask questions. We aim to make as certain as we can that each student comprehends what he is doing and why he is doing it. During laboratory sessions I never sit down at a table and read. On the contrary, I consider this the best opportunity I have during the whole week for individualized teaching.

2. My quiz papers and laboratory notebooks are never turned over to an assistant to be graded. By choice, I read all written work handed in by students. I consider this an important part of my job. I want to know at first hand how well each student expresses himself and the precise nature of his difficulties and misconceptions.

3. We have found that the Cooperative Physics Tests fit nicely into our testing program, but we use them only at the end of each semester for final examinations. For our monthly or triweekly one-hour quizzes, we use the question-and-answer or essay type because we want to find out whether the students can express their ideas in sentences.

4. I abhor the term "quiz section" and never conduct an oral quiz. For a teacher to sit with class book in hand, asking questions and recording a grade for each student, is in my judgment a dull performance and a perfectly pedantic way to quench every spark of student interest in a course.

5. I agree with O. H. Smith who, in his address[2] on this occasion a year ago, laid so much stress on the value of class discussion. It is remarkable how many difficulties can be resolved and how many concepts clarified in one hour if the teacher goes into the class room fully prepared to guide the discussion. Let me testify here and now that, even though I have taught general physics for forty years, to this day I still enter my classroom with an outline for the hour, not from an old notebook, but on a slip of paper written out for that particular meeting of the group. Physics is understanding, and it is a joy to see the face of a young man light up with enthusiasm when he grasps the meaning of one of Nature's laws. Time spent in preparation for teaching a class pays big dividends.

6. Excessive absences are often the fault of the teacher. It is a matter of record that in a class of twenty-five or thirty students, I have as a rule a total of no more than two or three absences in one semester for the whole group. The students know they are expected to come and to participate, and they can be depended upon. I have a feeling of disappointment and doubt when I hear a college or university teacher remark that he neither knows nor cares whether the students cut his classes or not, but that he holds them responsible only for passing his examinations at the end of the term. To my mind this attitude is not in the great liberal arts tradition.

I agree with H. A. Perkins[3] in his conviction that students should be encouraged to memorize certain algebraic statements and forms, that we should not frown on the use of memory in physics. For example, when a first-year student hears or reads the words "kinetic energy," he should think $\frac{1}{2}mv^2$; and when he hears or reads the term momentum, he should think mv. Without exact knowledge of the vocabulary of physics a student can neither read his textbook nor understand the professor's attempt to explain anything. Moreover, if he knows his laws and definitions he will not need three hours to write a one-hour quiz.

What I have said raises other questions. Should our courses not be student-centered? Are we teaching facts and subject matter, or are we teaching *students?* After all, is it not just a trifle immoral to teach subject matter nowadays? In my view a lot of nonsense has been written on this theme. I do not believe that we are faced with a dilemma here, and that we must choose one alternative or the other. Teaching the student and teaching the subject are inseparable aspects of the same phenomenon; and a good teacher, among other things, is a person who has a knack of making students learn.

Finally, is teaching physics an art or a science? Cheerfully and with a clear conscience, I straddle the fence and refuse to get excited about the answer to this question. Certainly a good deal can be learned about teaching physics, and to that extent it may be classed as a science. On the other hand, I believe that teachers, like students and poets, are born, and not too much can be done to alter the raw material. I have acquired a feeling of deep regret, as the years have gone by, that great teachers like Richtmyer and Watson, whose careers testify that teaching can be a real art, do not seem to be reproduced very readily in our modern educational system. I am glad to know that efforts are already being made at certain universities to rectify this situation, where any improvement will be welcomed not only by physics teachers but by the teaching profession as a whole.

[2] O. H. Smith, Am. J. Phys. **18**, 256 (1950).
[3] H. A. Perkins, Am. J. Phys. **17**, 376 (1949).

My research work takes up a good deal of energy, as I have to think out my programme for the next day every night.—LORD RUTHERFORD (as a young man).

An Automatic Chart Plotter for Lecture Room Demonstrations

Alfred O. Nier and R. B. Thorness
University of Minnesota, Minneapolis, Minnesota
(Received April 24, 1950)

A recording millivoltmeter is described which gives a record sufficiently large and distinct so that it can be seen by a large lecture audience. The device is useful for showing lecture room demonstrations in which currents or voltages vary with time and the audience needs a record to help appreciate the significance of the experiment. By means of connections to research apparatus in another part of the building, one may often perform actual experiments in front of a large class.

MANY demonstrations given in connection with lectures lose their effectiveness because the results of the experiments are not clearly presented. If the result occurs as an electric current or can be converted to one, it is customary for the lecturer to ask the audience to follow the excursions of a voltmeter needle or of a galvanometer spot projected on the wall of a darkened lecture room. While this suffices in some cases, there are many instances where the time variation or the complete record of the current is the interesting result of the experiment, and it is often too much to expect the audience to make an accurate mental time record of the galvanometer spot excursions. Strip chart recording milliammeters and self-balancing potentiometers are commercially available; but, since they are intended primarily for industrial or laboratory use, the recording paper is never more than 12 inches wide and the pen record extremely fine. Thus, if such an instrument is used for lecture demonstrations, the trace is seldom visible to more than the front row of the audience.

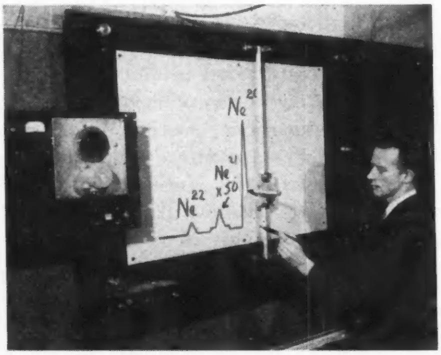

FIG. 1. Chart plotter suspended over blackboard in front of lecture room. Self-balancing potentiometer unit for driving pen in vertical direction is mounted to left of plotter. Lecturer used auxiliary pen for making explanatory notes on chart as the data are recorded.

The existence of an adequate automatic chart plotting device makes it possible for the lecturer to present in a better way some of the standard experiments now performed on the lecture table. Moreover, it is frequently possible to permit a class or audience to watch a *real* experiment being performed on a *real* research apparatus in another part of the building merely by running a pair of wires to the research laboratory.

The senior author is frequently asked to give lectures on some phase of mass spectrometry. Some of these are to classes in physics, others are to colleagues in other departments or to semi-popular audiences. It occurred to him that since it was neither practical to bring a large group to the research laboratory, nor the apparatus to the lecture room, one could do the next best thing and permit the audience in a lecture room to watch an experiment being performed by observing an automatic chart plotter operated from the actual apparatus in the research laboratory.

MECHANICAL DETAILS

Figure 1 is a photograph showing the chart plotter finally adopted. A plywood board $\frac{1}{2}$ inch thick and 54×40 inches hangs over the blackboard in front of the lecture room. Fastened to it by thumb tacks is a sheet of drawing paper, 37×29 inches. The vertical aluminum bar in the center of the picture is driven to the right at a constant speed by a small motor. A pen is

mounted on a carriage which moves up and down on the aluminum bar in response to the signal which it is desired to record. The illustration shows a mass spectrum of the isotopes of neon. A motor-driven potentiometer in a mass spectrometer[1] in another part of the building is in the process of tracing over the mass spectrum by increasing the accelerating voltage applied to the ions in the instrument.

Figure 2 is a schematic drawing of the mechanical details of the plotter. In Fig. 2(a) is shown the arrangement which moves the pen up and down. Pulley A is operated by the self-balancing potentiometer which measures the signal to be recorded. In Fig. 2(a), B, C, D, E, F, G, H, and I are all idler pulleys. The cable consists of a length of 0.007-inch diameter annealed tungsten wire. A spring S_1 is used to hold the wire taut. From the figure one can see that the vertical motion of the pen carriage is independent of the sideward motion of the main carriage, depending only upon the rotation of pulley A. A pair of taut tungsten wires 0.020 inch diameter, passing through holes in the pen carriage and fastened at their ends to the small horizontal bars which form part of the main carriage and support pulleys D, E, H, and I, act as guides to keep the pen carriage plane parallel to the plane of the board and prevent the pen carriage from turning due to friction of the pen on the paper.

Figure 2(b) shows the part of the arrangement which is used to provide the horizontal motion of the main carriage. A pulley J is coupled by a friction clutch to a slow speed motor which is used to provide the horizontal motion of the main carriage. As J rotates counterclockwise, the amount of cable between K and L increases by the same amount as the amount of cable between O and N decreases. A second cable (not shown in the interest of clarity) passes over pulleys R, S, J, T, P, and Q in succession and complements the cable shown. With the exception of J all pulleys in Fig. 2(b) are idlers. A counterclockwise rotation of J causes the main carriage to move to the right, always remaining vertical as it moves. A spring S_2 takes up the slack in the cable shown and a corresponding spring in the cable not shown keeps this cable tight. A taut horizontal tungsten wire 0.20-inch diameter passing through holes at the top of the main carriage and a corresponding wire passing through holes at the bottom of the main carriage act as a track and supports the weight of the entire carriage assembly. Figures 2(a) and (b) are, of course, the same figure. Certain parts have been omitted from (a) to clarify the illustration of other parts and *vice versa*. In practice, pulleys B and K, D and L, E and R, I and O, H and Q, C and P have common axles.

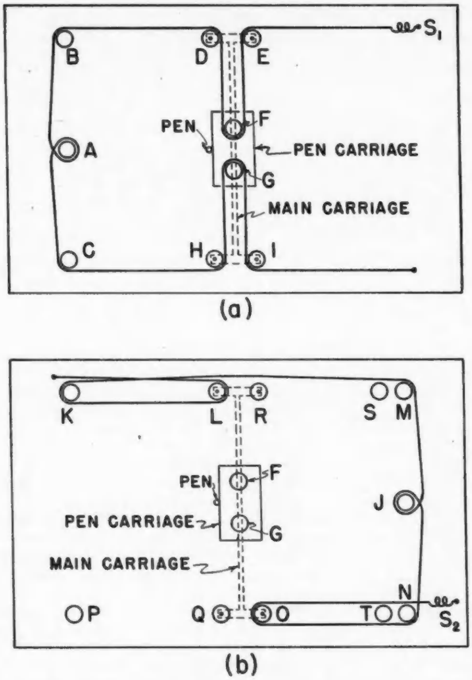

FIG. 2. Schematic drawing of essential parts of chart plotter. (a) Cable arrangement for moving pen carriage in vertical direction. (b) Cable arrangement for moving main carriage to left or right. All pulleys except A and J have groove diameters of 1.5 inches. Pulley A is $2\frac{3}{16}$ inches in diameter. Its periphery has a 48-pitch screw thread instead of a groove. It is $\frac{1}{2}$ inch thick and hence has 24 threads. This permits 6 turns of cable to be used to prevent slippage and allows enough extra threads to accommodate the cable as the pen carriage makes its full travel. Pulley J is $1\frac{3}{4}$ inches in diameter and, like pulley A, has a 48-pitch screw thread on its periphery. Since two cables must be driven by this pulley, it has 66 threads on its periphery and is $1\frac{3}{8}$ inches thick. Six turns of each cable are employed to eliminate slippage.

[1] A. O. Nier, Rev. Sci. Instr. 18, 398 (1947).

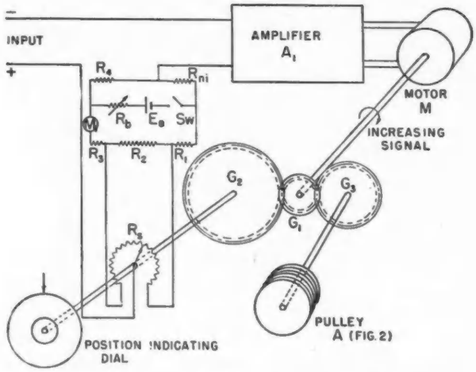

FIG. 3. Schematic drawing showing relation between electrical and mechanical components of self-balancing indicating potentiometer used to drive pen carriage. *A* Brown amplifier, Part No. 351921-1 complete with converter part No. 65829 (or equivalent); *M* Brown balancing motor, Part No. 76750-1 (or equivalent); G_1 gear, 32 pitch, 16 teeth (supplied with motor); G_2 gear, 32 pitch, 160 teeth; G_3 gear, 32 pitch, 24 teeth (chosen so that full scale on chart plotter corresponds with full scale on self-balancing potentiometer); R_s circular potentiometer such as Model 301S drum potentiometer, Helipot Corporation, Pasadena, California, approximately 50 ohms; M_1 milliammeter 0–5 ma; E_B dry cell, 1.5 volts, No. 6 size or equivalent; R_4 500 ohms; R_{ni} 5 ohms; R_b rheostat 100 ohms; R_1 2.5 ohms (adjusted); R_2 2.5 ohms (adjusted); R_3 250 ohms (adjusted). In constructing the circuit, R_3 was chosen as 250 ohms. Resistors R_1 and R_2 were adjusted so that when the input signal changed from 0 to 10 mv while the meter M_1 read 4 milliamperes, the position indicating dial reading changed from 0 to 100 divisions and the shaft holding *D*, R_s, and G_2 turned through about 325°. To change the range of the device the resistances R_2 and R_4 are changed. Full scale ranges as low as 2 millivolts can be employed without appreciable loss of precision.

THE DRIVING SYSTEMS

In the apparatus constructed, pulley *J* is coupled through a friction clutch to the output shaft of a 1/150-hp gear reducer motor whose output shaft turns at the rate of 2 rpm. Approximately seven minutes are required for a horizontal trip. Although the motor may be reversed, it is not convenient to wait seven minutes for a return trip. Thus, in practice, one raises the pen off the paper with one hand and pulls the main carriage back to the starting position with the other, the friction clutch permitting slippage. For some experiments a longer or shorter sweep time might be desirable. Thus, a simple arrangement to enable one to change gears in a gear reducer might be desirable.

In order to obtain the vertical motion two different systems were tried. In the first of these one of a pair of medium size ($5\frac{3}{4}$ in. long $\times 3\frac{1}{4}$ in. diam) war-surplus synchros was connected to pulley *A*, the second synchro being attached to the shaft of a 4.5-second Electronik[2] or Speed-O-Max[3] self-balancing strip chart recorder located at the point where the experiment is performed. While this arrangement performs remarkably well, it suffers from some defects. For example, since the error angle between the two synchros depends to a large degree upon the friction load placed upon the driven synchro, it was decided to choose pulley *A* of such size that 7 turns would be required to move the pen carriage from bottom to top, a distance of 24 inches. Thus, the synchro coupled to the recorder had to be geared to turn at a speed 7 times that of the shaft in the recorder. This placed a very large inertial load on the recorder and resulted in a marked lack of time response to sudden changes in deflection.

While the arrangement just discussed is completely satisfactory for many applications, the accuracy of reading and time response were not acceptable in all applications. In the second arrangement investigated pulley *A* was coupled directly by gears to the shaft of a self-balancing indicating potentiometer constructed from standard parts[4] of a commercial self-balancing potentiometer. In the particular unit constructed an input signal of 10 millivolts gave a full scale deflection, the time to reach full scale being 4.5 seconds. Figure 3 shows a schematic drawing of the unit.

The marking pen[5] used for making the trace resembles a fountain pen except that it employs a brush approximately $\frac{1}{4}$ inch in diameter in place of a pen point. The intensely black trace almost $\frac{3}{8}$ inch wide is clearly visible, from the back row of a lecture room having a capacity of 500 persons. The use of a marking pen has the disadvantage that a good grade of drawing paper

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[2] Brown Instrument Company, Philadelphia, Pennsylvania.

[3] Leeds and Northrup Company, Philadelphia, Pennsylvania.

[4] J. C. Mouzon, *Adaptability of the Measuring Circuit, Input Circuit and Amplifier of the Brown Electronik Potentiometer* (Brown Instrument Company, Philadelphia, 1948), Bulletin No. B15–10. This bulletin not only describes in detail the apparatus and circuit employed here but discusses self-balancing potentiometer circuits in general.

[5] "No-Mek" pen and ink obtainable from the Northern Products Company, 607 Pine Street, Stillwater, Minnesota.

must be employed to prevent blurring of the trace as the ink is absorbed by the paper. The marking pen has the advantage that the friction is extremely low. This is an essential feature if a synchro drive is to be employed. With the directly connected self-balancing potentiometer drive much more friction may be tolerated and a crayon or charcoal pencil could be employed. An inexpensive grade of wrapping paper might then be used.

In practice, several sheets of paper are thumb tacked to the board at the same time and as a record is completed the top one is torn off.

APPLICATIONS

In a one-hour colloquium talk on mass spectroscopy the senior author was able to perform some eight experiments showing appearance potentials and ionization efficiency curves of gases by electron impact, isotopes of various substances, gas analyses, and the use of a mass spectrometer as a leak detector. An assistant in the research laboratory operated the mass spectrometer and received instructions from the lecturer by telephone.

The apparatus should have many other applications. It could be used to show cooling curves of a substance undergoing phase changes. It would appear to be admirably suited to demonstrating characteristic curves of vacuum tubes. Because the main carriage can be returned quickly to zero by manual operation, a whole family of curves can be shown on the same sheet of paper. The operation of rectifiers and gas-filled tubes could easily be illustrated.

The construction of the plotter was aided by the Minnesota Technical Research Fund subscribed to by General Mills, Inc., *Minneapolis Star and Tribune*, Minnesota Mining and Manufacturing Company, Northern States Power Company, and Minneapolis Honeywell Regulator Company.

The Harvard Case Histories in Experimental Science: The Evolution of an Idea

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(Received November 14, 1950)

This is a discussion of the fundamental ideas behind the *Case Histories* prepared under the direction of President Conant for use in a general education course, Natural Science 4, at Harvard University. A limitation upon the results achieved in this program by lack of integration with other fields is pointed out. Integration of natural science with the humanities, social sciences, and biological sciences is established as a goal for general education. Lack of maturity among college students defeats in many cases the attainment of the goal, making necessary a whole-hearted effort to have the interrelations of the various fields constantly stressed in all.

ONE of the major revolutions in twentieth-century education is being led by the men of science. Paradoxically, through the application of the narrowly specialized techniques and attitudes engendered by compartmentalized training, they have come to see that the conventional categories under which they have worked are artificial in the extreme. In the forefronts of the laboratory they have found no true dividing lines between physics, chemistry, mathematics, astronomy, or biology. In psychiatry and psychology, too, they have seen that no man is totally a scientist or a humanist or a sociologist—but that all men are all of these in differing degrees at different times. Within the past two decades, then, leading educators and scientists have been pressing the point that education, if it is to fit men for efficient life in a scientific democratic state, must spread beyond its narrow and artificial categories and must educate both professional scientists and non-scientists in broader (actually more realistic) areas. One of the major problems of the modern educator is that of bringing back together the divergent streams of knowledge into a "unified field theory" of education. For the complex

twentieth-century world, a new scientific synthesis is sought which will realize for this age the "sound mind in a sound body" of the Greeks or the "well-rounded man" of the Renaissance.

Harvard University, under the able direction of its scientist-president, James Bryant Conant, has taken the lead in the "General Education" movement. The Harvard Plan, expounded in the "Report of the Harvard Committee"[1] is as widely discussed today as was the Chicago Plan a quarter century ago, and even more widely copied. Education, as the Harvard Committee saw it, after the last war, could no longer afford to concern itself solely with the specialized training which would fit a man to earn a living; it must also look "first of all to [the student's] life as a responsible human being and citizen."[2] In an age of science, this implied not only rendering the scientists more social and humane in their outlook, but also bringing the essence of the scientific disciplines to all students, even in nonscientific areas.

Undoubtedly this belief was fostered in Dr. Conant's mind during the years when he was called upon to work with governmental agencies and committees in exploring what science could do and could not do to further the war effort. As Conant himself later wrote, the nature of a democracy led to a peculiar state of affairs where "···national policy is determined by the interaction of forces generated and guided by hundreds of thousands if not millions of local leaders and men of influence···. Some understanding of science by those who shape opinion is therefore of importance for the national welfare···."[3] Not only was an understanding of science necessary for the efficient operation of a democracy in wartime, however; President Conant also felt, as have many other scientists, that there were values of a humane and philosophical type to be gained by its study. As he wrote in 1946, when his ideas on this matter had more crystallized,

> My argument, therefore, runs as follows: we need a widespread understanding of science in this country, for only thus can science be assimilated into our secular, cultural pattern. When that has been achieved, we shall be one step nearer the goal which we now desire so earnestly, a unified, coherent culture suitable for our American democracy in this new age of machines and experts.[4]

An opportunity arose for President Conant to work out his theories in more detail when he was invited to deliver the 1946 series of "Lectures on Religion in the Light of Science and Philosophy" under the Dwight Harrington Terry Foundation. These lectures, published in 1947 by the Yale University Press under the title *On Understanding Science*, contain the Conant philosophy of the new scientific education:

> Let me now be specific as to my proposal for the reform of the scientific education of the layman. What I propose is the establishment of one or more courses at the college level on the Tactics and Strategy of Science. The objective would be to give a greater degree of understanding of science by the close study of a relatively few historical examples of the development of science. I suggest courses at the college level, for I do not believe they could be introduced earlier in a student's education; but there is no reason why they could not become important parts of programs of adult education. Indeed, such courses might well prove particularly suitable for older groups of men and women···.[5]

Since it was the "Strategy and Tactics of Science" which President Conant desired to impart, rather than the latest scientific theories, he felt that a combination of the historical and philosophical approaches might be most fruitful. The student not intending to be a specialist in modern science could learn more general truths more rapidly if he were to consider selected discoveries in different scientific fields and in different ages. Dr. Conant suggested that for the purposes of this work the most ideal selections could be culled from the physics of the late seventeenth and eighteenth centuries, the chemistry and biology of the late eighteenth and early nineteenth centuries, and the geology of the early nineteenth century. As he explained his choices,

> The advantages of this method of approach are twofold: first, relatively little factual knowledge is required either as regards the science in question or other sciences, and relatively little mathematics; second, in the early days one sees in clearest light the

[1] *General Education in a Free Society* (Harvard University Press, Cambridge, Massachusetts, 1945).
[2] See reference 1, p. 51.
[3] James Bryant Conant, *On Understanding Science* (Yale University Press, New Haven, 1947), pp. 3–4.
[4] See reference 3, p. 3.
[5] See reference 3, p. 16.

necessary fumblings of even intellectual giants when they are also pioneers; one comes to understand what science is by seeing how difficult it is in fact to carry out glib scientific precepts···.[6]

For his Yale lectures, then, Dr. Conant, after a preliminary discussion of "The Scientific Education of the Layman," concentrated on two early and crucial periods of scientific investigation: (1) "Illustrations from the 17th century 'Touching the Spring of Air'" [primarily focused on Boyle, as the title would indicate]; (2) "Illustrations from the 18th century concerning electricity and combustion" [from Galvani and Volta through Lavoisier and Priestley]. He culminated his series with a summary of the basic principles demonstrated by these examples in a chapter called "Certain Principles of the Tactics and Strategy of Science," which lays down the philosophical framework governing science of the past and of today. If Dr. Conant's own definition of "science" be accepted ("···that portion of accumulative knowledge in which new concepts are continuously developing from experiment and observation and lead to further experimentation and observation···"[7]), there can be no question but that the approach and the selection of materials do lead to the desired end.

This proposed theoretical course fitted admirably into the Harvard Plan of Education—that of providing parallel, and to some extent competing and conflicting presentations of a general point of view. Dr. Conant took this opportunity, therefore, of supplementing the instruction in natural sciences already offered in Natural Sciences 1, 2, and 3, by instituting Natural Sciences 11a as a half-course at Harvard designed especially for juniors and seniors taking nontechnical curricula. His primary objective was not that of showing the interrelationships between all sciences (Natural Science 1), of presenting the salient facts of science (Natural Science 2), or of presenting a systematic history of science (Natural Science 3), but of using his case-history presentation to render the mode of thinking of the scientist intelligible to the nonscientist. As Dr. Conant expressed it,

··· I have in mind starting a young man who is not at all interested in science down the road which will end with his being a citizen—we hope a leading citizen—who can read about modern science and talk to modern scientists with some kind of understanding. I feel this should be the prime aim of the type of course with which I am concerned. To this extent the important thing is how one can convey to the student some idea of the point of view of the scientist in his laboratory, the role of hypothesis and theory, and the relationship between the deductive method of mathematics and the cut-and-try methods of experimentation of the empirical inventor or artisan. I believe that the case-history method is the nearest we can come to the more ideal procedure of having every leading citizen spend a year or two looking over the shoulders of research men in various kinds of laboratories which the modern world now supports···.[8]

In its original form, then, students were restricted to those taking no college work in science, but all of them had taken either high-school physics or chemistry. During the year, case histories were worked on in detail, using the admirable facilities of the Harvard library and its staff in the history and philosophy of science. Student questionnaires and opinionaires were utilized to determine student reaction, and the materials of the course were carefully analyzed from the point of view of their impact upon the students. The following year, 1948–49, this half-year course for juniors and seniors was reworked into a full year's course for freshmen and sophomores, and became one of the regular general education courses, Natural Sciences 4, The Growth of the Experimental Sciences, of the Harvard curriculum.[9] The case studies, especially prepared for this course, have begun to come off the Harvard University Press as the *Harvard Case Histories in Experimental Science.* Three case histories have already appeared: (1) *Robert Boyle's Experiments in Pneumatics*, edited by James B. Conant; (2) *The Overthrow of the Phlogiston Theory: The Chemical Revolution of 1775–1789*, also edited by Conant; and (3) *The Early Development of the Concepts of Temperature and Heat: The Rise and Decline of the Caloric Theory*, prepared by Duane Roller. A fourth, *The Atomic-Molecular Theory*, edited by Leonard K. Nash, has been announced for publication later this year by Harvard University Press.

[6] See reference 3, p. 18.
[7] See reference 3, p. 98.

[8] Personal letter, Dr. Conant to the author, November 17, 1950.
[9] *The Growth of the Experimental Sciences: An Experiment in General Education* (Harvard University Press, Cambridge, Massachusetts, 1949).

And, according to President Conant's progress report, *The Growth of the Experimental Sciences*, still another on "Pasteur's Work on Spontaneous Generation" is being prepared.[10]

For the layman or student not trained in the history or philosophy of science, these casebooks should prove most valuable reading. They are very carefully edited for this audience, with explanations, illustrations, and diagrams ample to enable anyone to follow the discussions of these early experimental scientists. For the historian of science, too, they are well worth their cost, for though they may sometimes irritate him by the elaboration of what to him is obvious, they do present in concrete and readable form the basic evidence and conclusions of some of the landmarks of science, and reprint the actual words of important texts which are difficult or impossible for the layman to obtain in their original form. Any publication of this sort which takes the reader back to primary sources is to be welcomed in a field which has all-too-frequently been entirely based on secondary or derivative materials. Most of our high-school and introductory college courses in science could be vastly improved by using one or more of these *Case Histories* as Dr. Conant is doing, both to place more emphasis on the methods of science and to mitigate in some measure the necessary dogmatism inherent in such introductory courses. Without question, President Conant is here proceeding in a scientific manner to handle philosophic ideas for the nonscientific, nonphilosophic audience. The experiment is a noble one; it deserves and will have real success.

Yet, though it may be presumptuous to criticize either the Harvard course Natural Sciences 4 or the ideas behind it on the basis of these printed documents, the social historian is likely to be somewhat disappointed by the restricted view of the development of science which in themselves they present. There is a brief attempt in each volume to place the experiment discussed into the theoretical background of its time, but there is little effort made to show the way in which it grew out of a particular need or a particular situation. As a result, the case histories by themselves at first glance appear to reinforce the nineteenth-century view of science as a series of steps in which the inherent genius of each great scientist advanced men's knowledge by bounds, rather than the twentieth-century view of science as evolutionary with many social and intellectual factors combining to contribute to—if not demand—the new discovery in its time and place. This is probably, however, the result of the academic situation in which the course is taught. The Harvard students who elect Natural Sciences 4 are not students of science, but students who by interest and training are primarily concerned with the humanities and social sciences. To capture their interest, it is necessary to start with social affairs and to work towards an interest in science and its impacts on society, and for this purpose a reading list of philosophical, social, and historical works supplements the *Case Histories*.[11] These form a necessary corrective for the deficiencies inherent in a study of the *Case Histories* taken alone, and if the reader is to obtain a valid picture of the strategy and tactics of science, he must take the two doses together, proceeding from social science to science if he is a social scientist, or from science to social science if he is a scientist.

Again, Dr. Conant has recognized the need for integrating scientific thought with social and political thought. By limiting the field, by selecting examples, he says:

> Political history, cultural history, the case history on pneumatics and the origins of organized scientific activity can be considered together···. We have by no means been as successful as we should be in achieving an integration of those diverse but inter-related elements. Skillful planning is required; as yet we are on only our second approximation. Indeed, the linking of the study of scientists to the study of science is by no means easy···.[12]

But to do all of these things in Natural Sciences 4 is well-nigh impossible. In this respect, with its general education courses limited to a single field, and with student election from among several courses within the separate fields, the Harvard Plan cannot possibly achieve this desired end of the integration of human knowledge. What Dr. Conant is saying is that the sociological and philosophical implications of science

[10] See reference 9, p. 20.

[11] See reference 9, pp. 11–13.

[12] See reference 9, p. 13.

are important, and that there is a unity underlying knowledge, whichever kind it may be. But he is assuming for the courses in science the duties which the general education courses in the social and humane studies should be filling. Integration of this nature can be achieved only when general education courses of the humanities, social sciences, natural sciences, and biological sciences are arranged into an integrated pattern of the whole, with each course of the sequence based on those which have gone before, and the interrelations of the various fields constantly stressed in all. Otherwise, each general education course becomes an entity in itself; each tries to do everything, and each ceases to be general education and is liable to become superficial in the attempt. Even at best, it is somewhat doubtful if integration can be achieved by instruction, since the integration of knowledge into a coherent whole is a by-product of a maturity which many college students cannot attain.[13] Yet if the attempt is to be made, and in such an important matter it must be made whole-heartedly, there should be a balance and a maturity in each segment of what is to be a balanced whole. The case-history method alone will not accomplish this, nor will these volumes reflect the balance that is sure to exist within the course at Harvard as Dr. Conant teaches it.

It might be much better, in this over-all view, if the case histories for Natural Sciences 4 were themselves more rounded in the tactics and strategy of science. The decision, probably a pragmatic one, to limit the materials to be considered to published works automatically excludes two very important sources of information about this field: manuscripts and machines. Science is not now, nor has ever been, the exclusive prerogative of the theorist; yet it is the scientific philosopher who has been the publisher of scientific treatises. Mechanics, craftsmen, and amateurs have done little publishing until recent years for obvious reasons; yet their developments must have influenced greatly the thoughts and experimentations of the theorists. But in spite of the tremendous interest in the backgrounds of modern science, no really sound work on the development of engineering or mechanical techniques has yet been published; no thorough study of machines and models has been made; no coordinated attack on unpublished scientific letters and manuscripts has been attempted. No complete course in the strategy and tactics of science can be given until much more fundamental research in these areas has been done.

The Harvard goal is integration of the humanistic, social, and scientific knowledge into a logical and coherent pattern which will be of real value to the citizen of to-day and to-morrow. Science alone cannot do the whole job. Cooperation and teamwork of all are the prime essentials to a sound program. All must contribute their special skills; all must work together in making their individual contributions to the whole. Certainly Natural Sciences 4 is a very real contribution to the education of the nonscientist expert, and as it progressesses it will come ever closer to answering the question which the layman wants and needs to have answered: "What is science?"

[13] See the interesting discussion on the possibilities of integration by Linus Pauling, "The place of chemistry in the integration of the sciences," Main Currents in Modern Thought **7**, No. 4, 108–111 (Winter, 1950),

Of all the services that can be rendered to science the introduction of new ideas is the very greatest. A new idea serves not only to make many people interested, but it starts a great number of new investigations···. There is nobody who has tested his ideas with more rigour than has Professor Rutherford. There can be no man who more nearly fulfils the design of the founder of the Nobel Prize than he does.—J. J. Thomson.

NOTES AND DISCUSSION

Heat-Sensitive Color Changes in Some Inorganic Chemicals

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AND
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THAT the iodides of mercury exhibit interesting reversible changes of color at certain temperatures is rather common knowledge among chemists. That other materials exhibit similar properties seems not so well known, at least to the workers of this century.

Houston[1] was sufficiently interested in the phenomenon to publish a list of about three dozen materials (including the iodides) which undergo a decided color change when heated and which return to the original color upon cooling. About a dozen others have been located, most of them in references near the turn of the century. These increase the list to over forty. The literature leaves much to be desired with respect to statements concerning the purity of the substances employed. In some cases contradictory experimental results are evident for the same material reported by different investigators.

As part of a preliminary survey of this phenomenon we were able to obtain commercially for examination thirteen of the materials referred to in the literature. These are of varied purity and no attempt was made to study the effect of this variation. It was felt that the effect of extremely small amounts of impurities would be so important that the desired degree of purity control would be impractical in the survey stages of the work.

TABLE I. Behavior of some temperature-sensitive materials.

Chemical	Original color	Final color	Transition temperature	Remarks
HgI yellow	yellow	orange-red	54° to 82°	Sensitive to light and air, gradual change of color, temperatures unreliable
HgI_2 (red)	(a) red (b) dark red	dark red yellow	48° to 127° 127°	(a) Gradual and slight change starting at 48°, with red deepening to dark red at 127° (b) Change of crystalline structure from tetragonal to rhombic
ZnO	white	pale green	Blue part of gas flame	Quite rapid change-over when placed in gas flame; above 250°C
CuI	gray-tan	orange	60–62°	Color change is slight and gradual. Poor reproduction of results
AgI	bright yellow	reddish-brown	145°	Rapid change-over at 145°, but long decay time not dependent on temperature, definite change of crystalline structure
Cu_2HgI_4	bright red	black	70°	Does not regain original color until temperature is lowered to 58°
Ag_2HgI_4	yellow	orange	47°	Sample tested was impure. Did not regain original color until 45°

Each of the materials was investigated carefully for temperature changes in the range 0 to 250°C and rough checks were made in a Bunsen burner flame for higher temperatures. The results of these and other tests are described in detail by one of the authors.[2]

Suffice it to say here that some of the materials failed to show any color change, some showed action different from that which had been recorded, and some checked nicely. We also were able to add one new material to the list.

The phenomenon of reversible color change is of interest for a number of reasons. First, it offers a field of study in solid-state physics which is relatively untouched. Second, it offers many possibilities of practical application. Temperature-sensitive paints are available which change permanently at the desired temperature. The materials described here (see Table I) react reversibly and may be used over and over again. Some of them react within a fraction of a centigrade degree. Others are not so critical, and some actually show a hysteresis effect, changing at one temperature on the rise and reversing at a different value upon cooling.

It seems not unreasonable that further work might make possible control over these materials such that the value of temperature for color change, extent of color change and other variables could be adjusted as desired. The problem is not unlike that of phosphors which can now be prepared with considerable control of the characteristics. The sensitivity of phosphors to impurities is well known, and our work would indicate a similar sensitivity in the color changes.

Table I shows the materials which yielded rather consistent results in our survey. We have found numerous uses for these materials as indicators around the laboratory. When mixed with a vehicle such as clear lacquer or Glyptal they may be used as indicator paints in locations where thermometers or thermocouples are impractical for reasons such as excessive heat capacity or poor thermal contact. In one application we were able to make a rather accurate study of the end-effect cooling of a delicate heater wire.

Two applications as vacuum gauges or indicators have been tried with some success. Thermocouple gauges and Pirani gauges depend upon the cooling effects of gas upon current-carrying heater wires. In the thermocouple gauge the temperature variations with pressure are measured with a thermocouple. In the Pirani gauge the changes in resistance of the filament are measured and correlated with pressure.

If a small section of ribbon filament is coated with a temperature-sensitive pigment, the heating current necessary to produce the critical temperature for color change is a function of pressure. The external circuit is simple, consisting of an ammeter and adjustable current source.

In a device indicating only ranges of reduced pressure, a number of pigments critical to different temperatures are painted on the filament. At constant filament current, as the pressure is reduced, the wire becomes hotter, and one after another the different pigments change color. This arrangement indicates only that a certain temperature (or pressure) has been reached. Although it worked, this scheme was not as useful as the single indicator arrangement described in the preceding paragraph.

[1] J. Houston, Chem. News **24**, 177 (1871).
[2] J. B. Maginnis, unpublished thesis, Syracuse University (1948).

Freezing in Water Pipes

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IT is widely known that water expands considerably in freezing (about 9 percent in fact) and that this expansion is commonly believed to be the immediate cause for the rupturing of a pipe, which frequently, but not always, results from such freezing. This belief would be justified if the freezing segment were strictly confined by barriers, in the form of closed valves at the two ends of the freezing segment, but actually the freezing section is a rather short length with longer portion at each end in which the liquid remains liquid. For example, consider a horizontal stretch of pipe in an ordinary dwelling, so situated that a short segment of it, say a couple of feet, is exposed to air at a temperature well below 32°F. Under ordinary circumstances, the pipe on one side of this segment will be directly connected, without an intervening closed valve, to the underground water main. Let us call this side the side A. The other side, which we call side B, will be terminated, so far as we are concerned, by a closed faucet somewhere in the house.

The freezing starts in this way: first a thin layer of tiny ice crystals forms on the inside surface of the pipe, along the cold segment, for that is where the coldest part of the whole water mass is. As the process goes on, more ice crystals are laid down against the first layer, causing the layer to increase in thickness, making a sort of *sleeve* of ice, with its outer surface firmly cemented to the inner surface of the pipe. Eventually, the layer will thicken so much that the channel through the middle vanishes, and the sleeve becomes a solid core, which we will call an ice plug. Any further freezing will only lengthen the plug.

Up to the moment when the channel is completely closed, side A and side B are in direct communication with the water main, and the water in each will be automatically at the same pressure as that in the main. Of course, every ice crystal, when it forms, suffers the 9 percent expansion, but this expansion is taken care of by pushing back the water. This, of course, means that a certain amount of water, a very small amount, is pushed back into the main.

After the channel is closed, this condition still holds for side A, but not for side B, which is then cut off from communication with the main. Meanwhile, the diameter of the ice plug has not increased at all, since it has all the time been fixed by the diameter of the pipe. The plug has grown only by accretions of more ice crystals, on the inner surface of the channel and at the ends. After the channel is closed, the growth can go on only at the ends. One side A this growth causes no trouble, since the extra volume required is supplied by pushing more water into the main, but on side B the result will be a rise in pressure of the liquid. Breakage may be prevented by leakage at the faucet which terminates B. Also, elasticity in the metal of the pipe relieves the pressure to some degree. But if the pressure rises enough, the pipe will undoubtedly break. Where? Wherever the pipe is weakest, not necessarily at the ice plug but more probably somewhere else, always on the B side.

Much light can be thrown on these phenomena by the simple experiment of freezing water in a thin-walled glass test tube. The size of the tube is not important, but one about ½ inch in diameter and 6 or 8 inches long does very well. Pour tap water into it up to a height of 4 or 5 inches, and carry out the freezing by means of a freezing mixture of water, ice, and salt, in a beaker. If only the closed end is first inserted, allowing ice to form there, and the tube is gradually lowered step by step until the whole liquid content of the tube is frozen, the process goes on quietly without damage to the tube. The formation of crystals against the inside wall of the glass, the subsequent thickening of the walls of the sleeve, and the completion of the ice plug from the closed end to the top can be watched. While this goes on, the level in the tube will rise perceptibly, so that if the original length of the water column was 5 inches, the final length of the cylinder of ice will be nearly 5½ inches. The ice may then be melted and the experiment repeated with the same tube.

If the cooling is started by inserting the whole tube in the freezing mixture up to the level of the water inside, the whole experiment fails. Unless a freezing mixture is continually stirred, the top part of that mixture becomes considerably colder than that below it, owing to the ice in the mixture floating to the top. The result is that a solid plug of ice forms at the top of the column in the tube, extending clear across the tube, and sealing off the water below where the water has been only partially frozen. This plug acts like a closed valve, so that the further freezing of the water below it develops a high hydrostatic pressure and the tube shatters.

The first technique, gradual freezing from the bottom up, illustrates what goes on in the A side of a water-pipe. The closed end of the tube corresponds to the center of the ice plug in the iron-pipe case, and the atmospheric pressure on the top of the column takes the place of the water-main pressure at the end of the iron pipe.

On the other hand, the second technique, after the short ice plug at the top of the column has been formed, corresponds to what occurs on side B, as can easily be seen.

In most suburban and city houses, there is a setup which closely resembles the case of a test tube in which the freezing starts at the closed end and proceeds continuously to the other end. This is the arrangement for watering an outside garden or lawn. A pipe connected with the water main extends through the basement wall and is terminated by a faucet close to the wall on the outside, which is

threaded so that a garden hose can be attached. There is a cut-off valve inside the basement a foot or two from the hose-faucet which can be closed in an emergency, but ordinarily should be left open, so that there is no barrier between hose-faucet and main. The only freezing that can occur will start at the hose-faucet and proceed along the pipe into the house, not more than a foot or two, if the house is occupied and heated.

The original purpose of the cut-off valve evidently rested on the assumption that, on the approach of winter, the householder would close it, open the hose-faucet and drain off the water between them to prevent freezing. But it is quite clear that if this is not done, the water near the end of the pipe will indeed freeze, but no harm will result. Probably many householders forget this precaution anyway, and think they are very lucky in not having a burst pipe. The writer of this article has for about twelve years, in three different hourse, purposely let his cut-off valve alone and not suffered any inconvenience thereby. However, the cut-off valve would be useful if it became necessary to replace the washer in the hose-faucet.

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The Fall of a Particle through the Atmosphere

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In a recent letter to the editor of this Journal,[1] a request was made for solutions, in closed form, of the equations

$$\ddot{y}=g-ke^{\beta y}\dot{y}, \tag{1}$$

and

$$\ddot{y}=g-ke^{\beta y}\dot{y}^2, \tag{2}$$

where g, k, and β are constants and the dot signifies differentiation with respect to time.

Since $\beta\neq 0$, Eq. (1) can be written in the form

$$d\dot{y}/dt=(d/dt)(gt)-(k/\beta)de^{\beta y}/dt. \tag{3}$$

A first integral of this is

$$dy/dt=a+gt-(k/\beta)e^{\beta y}, \tag{4}$$

where a is a constant of integration. If both sides of Eq. (4) are multiplied by $-\beta e^{-\beta y}$, one obtains

$$(d/dt)(e^{-\beta y})+\beta(a-gt)e^{-\beta y}=k. \tag{5}$$

If the substitution $s=e^{-\beta y}$ is made, Eq. (5) becomes

$$(ds/dt)+\beta(a-gt)s=k, \tag{6}$$

the solution of which is

$$s=\left[k\int_0^t \exp\{\beta(at+\tfrac{1}{2}gt^2)\}dt+b\right]\exp\{-\beta(at-\tfrac{1}{2}gt^2)\},$$

or

$$y=\tfrac{1}{2}gt^2+at-(1/\beta)\ln\left[b+k\int_0^t \exp\{\beta(at+\tfrac{1}{2}gt^2)\}dt\right], \tag{7}$$

where b is an arbitrary constant.

Equation (2) can be solved by inverting the dependent and independent variable. When this is done,

$$d^2y/dt^2=-[1/(dt/dy)^3]d^2t/dy^2, \tag{8}$$

and Eq. (2) becomes

$$d^2t/dy^2=ke^{\beta y}(dt/dy)-g(dt/dy)^3. \tag{9}$$

If $s=(dt/dy)$, Eq. (9) becomes

$$ds/dy=ke^{\beta y}s-gs^3. \tag{10}$$

This equation is of the Ricatti type and can be solved by using the substitution $s=\phi^{-\frac{1}{2}}$. Then,

$$(d\phi/dy)+2ke^{\beta y}\phi=2g, \tag{11}$$

which has the solution,

$$\phi=\left[a+2g\int_0^y \exp\{(2k/\beta)(e^{\beta y}-1)\}dy\right]\times\exp(-2k/\beta)\{(e^{\beta y}-1)\}, \tag{12}$$

where a is a constant of integration. However $\phi=(dy/dt)^2$, and thus, substituting for ϕ from Eq. (12), one obtains a differential equation of first order whose solution is

$$t=b+\int_0^y \frac{\exp\{(k/\beta)(e^{\beta y}-1)\}dy}{\left[a+2g\int_0^y \exp\{(2k/\beta)(e^{\beta y}-1)\}dy\right]^{\frac{1}{2}}}, \tag{13}$$

where b is a constant of integration.

Equation (13) can be represented graphically as follows: Noting that

$$\int_0^y \exp\left\{\frac{2k}{\beta}(e^{\beta y}-1)\right\}dy=\frac{e^{-2k/\beta}}{\beta}\int_{2k/\beta}^{(2k/\beta)e^{\beta y}}\frac{e^x}{x}dx, \tag{14}$$

then

$$\frac{\exp\{(k/\beta)(e^{\beta y}-1)\}}{\left[a+2g\int_0^y \exp\{(2k/\beta)(e^{\beta y}-1)\}dy\right]^{\frac{1}{2}}} \tag{15}$$

can be determined for all values of y by a graphical evaluation of Eq. (14).

If Eq. (15) is plotted against y, the area under the curve between $y=0$ and y gives the value of $(t-b)$ according to Eq. (13) for every value of y.

[1] R. H. Bacon, Am. J. Phys. 19, 64 (1951).

Motion of a Particle through a Resisting Medium of Variable Density

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In a recent communication Bacon[1] requested solutions of the differential equations

$$\ddot{y}=g-ke^{\beta y}\dot{y}, \tag{1}$$

$$\ddot{y}=g-ke^{\beta y}\dot{y}^2. \tag{2}$$

For convenience, change to dimensionless variables

$$Y=\beta y,\quad T=(\beta g)^{\frac{1}{2}}t,$$

giving

$$\frac{d^2Y}{dT^2}=1-\alpha_1e^Y\dot{Y},\quad \alpha_1=k/(\beta g)^{\frac{1}{2}}, \tag{1a}$$

$$\frac{d^2Y}{dT^2}=1-\alpha_2e^Y\dot{Y}^2,\quad \alpha_2=k/\beta. \tag{2a}$$

Equation (1a) can be integrated once to give

$$\dot{Y}=(T+A_1)-\alpha_1e^Y. \tag{3}$$

By using the substitution

$$Y=-\ln Z \tag{4}$$

Eq. (3) is transformed to a linear equation

$$\dot{Z}+(T+A_1)Z=\alpha_1. \tag{5}$$

Solving Eq. (5) and taking the logarithm yields

$$Y=\tfrac{1}{2}T^2+A_1T-\ln\left[A_2+\alpha_1\int_0^T \exp(\tfrac{1}{2}Z^2+A_1Z)dZ\right]. \tag{6}$$

The integral can be reduced to a tabulated form by completing the square in the exponent and changing variables.

The Eq. (2a) is reduced to a first-order equation by the standard device

$$p=\dot{Y},$$

$$pdp/dY=\frac{d^2Y}{dT^2}.$$

The resulting equation,

$$p(dp/dY)+\alpha_2e^Yp^2=1, \tag{7}$$

is linear in p^2:

$$(dp^2/dY)+2\alpha_2e^Yp^2=2, \tag{8}$$

and can be solved for $p(Y)$. Then the solution of Eq. (2a) is

$$T-T_0=\int_{Y_0}^{Y}dY/p(Y). \tag{9}$$

Munk[2] has solved the problem for the velocity as a function of the density and plotted his results. In our notation his solution of Eq. (8) is

$$p^2=e^{-2\alpha_2\exp(Y)}[A+2Ei(2\alpha_2e^Y)], \tag{10}$$

where $Ei(x)=\int(e^x/x)dx$ is a tabulated function. This reduces the second problem to a single numerical integration.

The interpretation of Eq. (1) should be modified, k being the viscosity rather than the density. This is a special case of the relation derived by dimensional analysis that the drag per unit area is proportional to $\rho^{n-1}\mu^{2-n}v^n$, where ρ represents density, v, velocity, and μ, viscosity. The NACA Standard Atmosphere[3] has a small linear variation of viscosity from 0–35,000 ft and constant viscosity above 35,000 ft. Because of the very small variation, the linear relation can be approximated by an exponential.

[1] R. H. Bacon, Am. J. Phys. **19**, 64 (1951).
[2] M. Munk, Aero Digest (February 15, 1944).
[3] W. S. Aiken, NACA Technical Note 1120 (September, 1946).

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ANNOUNCEMENTS AND NEWS

Book Reviews

The Principles of Cloud-Chamber Technique. J. G. Wilson. Pp. 131, Figs. 33, 9 × 5½ in. Cambridge University Press, New York, 1951. Price $2.75.

In the past there has been no suitable reference that could, without serious reservations, be recommended to a beginner in the field of cloud-chamber observations. I feel quite sure that each of us who has used cloud chambers has always intended to write a book on the subject, if only to reduce the volume of word-of-mouth instruction of our own students. Fortunately, Wilson's book will now solve the problem.

The author has purposely omitted detailed discussions of design and operation, but instead has concentrated on general considerations common to nearly all design and operation. Wilson gives a scientific reason for this decision, but he may also have justified the decision in his own mind from the observation that physicists are usually rugged individualists when it comes to design of experiments.

It is impossible to make such a book completely up-to-date at the time of printing; in this case, the results of renewed interest in diffusion chambers are thus omitted. The work of C. E. Nielsen and his collaborators on topics such as condensation on ions, drop-image resolution, and measured ionization *versus* velocity in the nonrelativistic realm, is not discussed, since it is unpublished. Fortunately, according to Nielsen, these important contributions will soon appear in print.

In general, the monograph is authoritative, very readable, and tolerably complete. Illustrative photographs might have added greatly to its usefulness for the uninitiated, but apparently Wilson is planning a new edition of the atlas of cloud chamber pictures as a companion volume.

Wayne E. Hazen
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From the Life of a Researcher. William Weber Coblentz. Pp. 238 + xi. Philosophical Library, New York, 1951. Price $4.75.

Here is a researcher whose early experiences taught him that his best performances would probably be found by utilizing his inventiveness, manipulative skill, ability to work efficiently and therefore rapidly, persistence, and physical strength. In this manner he hoped to make the most of his overwhelming desire to increase knowledge in physics. He chose a narrow specialty and, after one little excursion, never allowed himself to be attracted to new or startling ideas far afield. One might classify his life interest in the Bureau of Standards as radiometric instruments and methods. Unquestionably, he reaped unusual rewards by a long sustained and uninterrupted effort in a familiar field.

Dr. Coblentz does not seem to enjoy wearing his medals. In *Who's Who* can be found a record of six medals or equivalent high honors, but the reviewer failed to notice any reference to them in the autobiography. He mentions

his membership in the Academy of Sciences incidentally, but never refers to the fact that for many years he was the only Academician at the Bureau. One of his chapters was of necessity devoted to his physical investigations and some applications. A glimpse of the man is had by his comment after twenty pages of this description. "Thus endeth the summary of 'accomplishments' of practically a one-man-and-assistant laboratory. Some of my flatterers seem to think it was a fairly good show. If so it was not through any great brilliancy, but through the long steady pull of an old work horse. . . Especially in my younger days, how I used to wish that I had been endowed with more brains and less brawn. Then, with adequate assistance (which I never had) I could really have accomplished something."

The book contains eighteen chapters, covering family history, a large number of details of his early life, struggles for an education, student life, physical investigations, and numerous sidelights on his life, concluding with a personal chapter on "The green flash at sunset." Therein are many interesting glimpses of the man and his experiences. Not the least is the account of a meeting of the Philosophical Society of Washington when Alexander Graham Bell's paper was followed by one from Dr. Coblentz which contained some apparently contrary evidence on the photoelectric response of selenium. The great inventor walked out on Dr. Coblentz, punctuating his stride with the thump of his cane. Without recounting the incident in full, it should be said that the differences in results could be harmonized.

Physics research of today differs greatly from that which Dr. Coblentz observed when he found his place in life. Indeed, there is a great contrast in problems, in methods, in team research, and in opportunities. So the thoughtful prospective physicist will find in this life of a researcher not an example of what can be done again, but yet much that will prove very suggestive for his own orientation and the best utilization of his talents. Then, too, the historian of science needs such records.

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The Friction and Lubrication of Solids. F. P. BOWDEN AND D. TABOR. Pp. 337. Oxford University Press, New York, 1950. Price $7.00.

In this monograph the authors discuss chiefly the results of their own experiments and those of their collaborators in Melbourne, Australia, and Cambridge, England. They have made a significant contribution toward the clarification of our understanding of the mechanism of friction and boundary lubrication and have applied their findings to the improvement of machine shop operations and running machinery. The first eight chapters are of special interest to a physicist, the second eight to an engineer.

Coulomb suggested that friction is caused by molecular attraction; but discarded this hypothesis because he found the frictional force between two sliding surfaces is not dependent upon their area, as the theory led him to expect. However, modern experiments show that frictional force *is* proportional to the area of actual contact rather than the area of coverage as used by Coulomb. If we adopt this definition of area, the molecular attraction hypothesis becomes a fruitful one. Some investigators have already taken steps toward making it a well-established theory. The present authors regard molecular attraction as the fundamental phenomenon of friction; hence, it is the predominant theme in this book.

We learn that two polished metallic surfaces in contact touch only at the peaks of their roughnesses, which may be approximately one micron in height. The peaks are crushed and yield plastically until their total area is sufficient to support the load, to which it is roughly proportional. The peaks of one surface become welded to those of the other so that when a force, the frictional force, produces sliding, the peaks are sheared off and also plow up the surfaces. These develop hot spots at the points of actual contact which may cause softening or even local melting. Bits of one surface may be plucked away and adhere to the other surface, thus causing physical damage or wear. Clean outgassed metal surfaces adhere strongly; but the friction between them is greatly reduced by a layer of absorbed gases, of oxides, or of a soft metal. The friction of many nonmetals, such as sapphire, mica, some plastics, and glass is similar in mechanism to that of metals, whereas that of other materials, such as graphite, Teflon, rubber, and wool fibers, is not.

Boundary lubrication occurs when the thickness of the lubricating film is of molecular dimensions and is penetrated here and there by peaks of metallic roughnesses which form local welds, as when the surfaces are unlubricated. Hence, the frictional force depends upon the nature of the surface as well as upon the chemical constitution of the lubricant. At low speeds and under heavy loads the friction coefficient of paraffins, alcohols, and fatty acids decreases with molecular weight (chain length) of the lubricant down to a minimum value of about 0.07. It is independent of surface finish. It is also independent of temperature up to a transition point above which the friction increases. This point is usually the melting point of the lubricant and hence of the boundary film. The fatty acids lubricate most effectively only when they react with the metal to form a soap. Such a soap film has strong lateral cohesion and a softening point higher than the melting point of the pure acid. A single molecular layer of soap may provide good lubrication even on a rough surface, whereas the same effect is produced by metallic films and those of solid hydrocarbons and alcohols only at a greater thickness (*ca* 1×10^{-5} cm).

In extreme pressure lubrication, the chemical reaction of the additive with the surface is of great importance. Since such a reaction may not occur until a certain temperature has been reached, lubrication below this temperature is achieved by combining the extreme pressure lubricant with a fatty acid. Corrosion will be prevented if the reaction product reduces intermetallic contact at the operating temperature.

The authors have been most successful in compressing a vast amount of well-illustrated material into approx-

imately 300 pages with a minimum of mathematics and without sacrificing an easy, readable style. They have presented an overwhelming mass of evidence in support of the theory that the fundamental phenomenon of friction is molecular attraction, or adhesion. They conclude with a discussion of the nature of metallic wear, adhesion between solid surfaces, and chemical reaction produced by friction and impact.

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Applied Nuclear Physics. Second Edition. ERNEST POLLARD and WILLIAM L. DAVIDSON. Pp. 352. John Wiley and Sons, Inc., New York, 1951. Price $5.00.

The first edition of this book was published in 1942 at a most opportune time, when an authoritative and readable survey of nuclear physics was needed to introduce the many new workers in the field to its fundamental ideas. The second edition maintains the excellence of the previous one and is somewhat expanded to cover the developments through and after the war, and certain of the older chapters are rewritten in places to give greater clarity.

The authors have performed an outstanding service in their approach to the wartime developments of atomic energy. While the vast majority of popular and semipopular books on nuclear physics have dwelt at great length on nuclear energy as a destructive force, these authors scarcely mention an atomic bomb. Instead, they try constructively to put the various subjects of nuclear physics in their proper prospective.

It seems to be a common assumption that all efforts to show the adult public the difference between nuclear physics and atomic warfare have failed so miserably that it is hopeless to try further. Many argue that the only solution is to educate the coming generation to appreciate the tremendous potential value of nuclear energy for the public welfare. Without depreciating the worthwhile effects in this direction in our preparatory schools and high schools, a defeatist attitude toward adults is hardly constructive. Such treatments as given by Pollard and Davidson should do much toward emphasizing the importance of nuclear physics as a tool and as a source of energy for our everyday living.

The stamp of Professor Pollard's profession was shown in the first edition by an appendix which included some elementary problems in nuclear physics. This has been replaced in the second edition by an appendix including some laboratory experiments. If the book goes to a third edition, this feature should be eliminated completely, since so little real description of the apparatus is given that one could not set up a laboratory from such a treatment. Where the apparatus is useful and illustrative, a description of it could well be incorporated into the test.

In their preface the authors state: "In treating the subject of nuclear physics the technical aspect is emphasized in this book. We aim at presenting the essential facts and methods of artificial radioactivity and transmutations in such a way as to be of service to the growing array of chemists, biologists, physicians, and engineers, who, though not necessarily versed in the language of physics, are using the products of nuclear physics to further their ends in their own sphere." They have succeeded admirably in this aim.

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The Heavens Above, a Rationale of Astronomy. J. B. SIDGWICK; American edition prepared by WARREN K. GREEN. Pp. 333. Oxford University Press, New York, 1950. Price $4.00.

What sets this book apart from the general run of popular writings on astronomy is the avowed purpose, in the words of the author, "to give a systematic demonstration of the more complex facts of Astronomy, starting from simple assumptions at which not even the most skeptical reader could cavil." The degree of success of such a program can be determined, no doubt, only by a measure of the skepticism of the most skeptical convert. However, a few general comments may be in order.

There is a branch of astronomy concerned with the motions of the planets, celestial mechanics, which is, perhaps, unique in science—unique in the sense that the underlying mechanism may be explained in terms accessible to anyone with the least degree of mathematical sense. Further, when applied to the motions of the planets, the theory predicts observations with almost unbelievable accuracy when the scale of the motions and the length of the time scale are considered. In witness of the accuracy of the theory, consider the temerity of the U.S. Naval Observatory in publishing the predicted positions of the planets to one-hundreth of a second of arc years in advance of the event.

In exploiting the successes of the newtonian celestial mechanics the author of *The Heavens Above* proceeds in a careful and convincing fashion from the elementary observation of diurnal rotation, and the apparent motions of the planets, through the false constructions of the ancients to the triumphs of Copernicus and Kepler. It would be a skeptical reader indeed who held to any form of geocentric universe after working over the first five chapters of this book.

Unfortunately, the situation in modern astrophysics lacks those features which made possible such an agreeable presentation of the achievements of classical astronomy. The data of observation are accessible only through relatively complicated devices: the photographic plate, the spectroscope, the photocell, to mention a few. The interpretation of the data obtained through these devices depends in turn on a profound knowledge of their normal and eccentric behavior and on the state of existing physical theory. In the attempt, apparently, to cover as much ground as possible, the author slurs over these difficulties in order to focus the reader's attention on the phenomena. As a result, the reader, skeptical or not, is forced to lean more and more on the author; the fundamental data are taken as given.

Since this is the case, it seems that a gain in clarity might be obtained if a less argumentive approach were

adopted. As an example, the discussion of the structure of the galaxy in Chapter 5 would proceed much more smoothly if the evidence from the motions of the stars were introduced at the start. As it is, the whole topic of stellar motions, galactic rotation included, is treated in a rather cursory fashion.

On the credit side it may be said that the aim of the book is high, the author's style makes for easy and pleasant reading, and a surprising amount of material is covered, although much of it superficially.

A more liberal use of diagrams would be helpful; those used are often rather unrevealing, e.g., Fig. 50, the Russell diagram; there is no photograph showing lunar craters; surprisingly, there is no photograph or sketch of any telescope.

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Crystal Growth. H. E. BUCKLEY. Pp. xv+571. Illustrated. John Wiley and Sons, Inc., New York; Chapman and Hall, Ltd., London, 1951. Price $9.00.

Recent rapid advances in the art of growing large, single crystals for piezoelectric purposes have emphasized the extent of the problem of crystal growth. So also has research activity in other directions. In the solid-state field, vigorous efforts are being made to understand and evaluate the molecular and ionic forces in crystals. The useful information on the subject of crystals and their growth is widely scattered in the literature, and those interested in this fascinating study should put this book on their reading list. The author is to be congratulated on his effort to correlate the great mass of information from various fields.

This reviewer has been impressed with the variety of viewpoints held by workers in different parts of this field, particularly as to the perfection of the crystal structure. The geologist appears to be most happy when examining an imperfect crystal, and each crystal is an individual project. The engineer, concerned with the production of many large, single crystals, for electrical or other purposes, all as nearly perfect as possible, views even the slightest defects with a jaundiced eye. At the risk of oversimplification, it might be said of the solid-state investigator that he is concerned with imperfections in the crystal structure which are not visible to the eye. It is time that an expert in the field of crystal growth should make a first attempt to correlate these widely divergent viewpoints.

The author of the book is considered an authority on the subject, with special emphasis on crystal habit and the modifying effects of impurities. He is well versed in the peculiarities which are often found in this highly organized state of matter. To summarize the contents of the book, the subject is introduced with an excellent discussion of solubility, and the effect of particle size on this factor. A clear treatment is also given of the metastable range of supersaturation in which controlled crystal growth takes place. A comprehensive analysis follows of most of the recognized methods of growing crystals; and the reader is well prepared to consider the many theories of crystal growth and related topics, which comprise the mid portion of the book. It concludes with several chapters on the author's particular specialties, and an appendix is given on specific examples of habit modification by impurities which should prove of value to any one interested in growing crystals.

It is doubtful if a single writer could possibly do full justice to this extensive and confusing subject unless he were geologist, crystallographer, production engineer, experimental and theoretical physicist all rolled into one. The care with which the book has been assembled is attested to by the fact that it was planned before World War II and has been in preparation for many years.

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Adventure into the Unknown. LAWRENCE A. HAWKINS. Pp. 150 × ix, Illus. 26. William Morrow and Company, New York, 1950. Price $3.50.

The author tells a brief, interesting story of achievements connected with the General Electric Company's Research Laboratory since its organization in 1900 with temporary quarters in a barn in Dr. Steinmetz's backyard. Because of his thirty-six years' association with the laboratory, Mr Hawkins is well qualified to write on the subject.

Founded in 1892, out of a merger of two existing companies, the General Electric Company entered the depression of 1893. The electrical industry was then in its infancy, and industrial potentialities were based on the fundamental researches of Michael Faraday and his contemporaries. Very little fundamental research was undertaken in this country previous to 1910, and most of what was being done was found in the universities and the research institutes.

The author points out that the responsibility for the decision to establish an industrial research laboratory, the nation's first, fell chiefly on such well-known men as Elihu Thompson, Charles P. Steinmetz, E. W. Rice, Jr., and others. After surmounting the difficulties incident to inducing the directors to form such a laboratory, they planned an over-all research strategy which included the improvement of products and a program of basic research.

The book is dedicated to Dr. Willis R. Whitney. Since the success of a research laboratory depends so much upon the director, the choice is commendable. Dr. Whitney believes thoroughly in the experimental method of approach. A theoretical study to him is good if it points toward an experiment. His aim was to give increased freedom in laboratory methods and in the choice of research work, depending entirely upon ability. Dr. Langmuir's success with the company illustrates this very well; i.e., he was given considerable opportunity to look around to see if there was anything he wished "to play with." This method produced very fruitful results.

Limited space is given to Dr. Katherine B. Blodgett, who has been before the public many times in the last fifteen years. Apparently, she was the second woman

scientist in the General Electric Laboratories, entering at a time when women generally were not wanted in such work. Starting as an assistant to Dr. Langmuir for research on films, she received considerable publicity in 1938 when invisible glass was introduced as a result of her studies. She is now one of the best-known women scientists in the country.

There is a brief description of the present laboratory located about five miles east of Schenectady and known as the Knolls Research Laboratory. It was dedicated in October, 1950. The main building is designed for extreme flexibility in space and facilities. It has readily movable partitions so that the rooms may be changed in size to suit any new experimental conditions. Piping and electrical services are designed to accomodate themselves to this type of structure. Special buildings are designed to house large physical equipment such as the betatron, synchrotron, high voltage x-ray equipment, and cryogenic laboratory. Thus, the reader can see that the laboratory has gone heavily into nuclear work.

New cooperative research work between the government and the General Electric Laboratories on atomic energy projects is described. Much of this work is done in another large twenty-six-million-dollar laboratory completed in 1950 and known as the Knolls Atomic Power Laboratory, located about half a mile east of the Knolls Research Laboratory.

The scientist will enjoy the book because it reviews the development of one of our best-known industrial research laboratories and the success of outstanding scientists connected with it. The lay person can get a very clear picture of outstanding achievements in this field of science and of the traits of men who have done the work. The book will have value in high school and public libraries because of the success stories. The author calls our attention to the fact that two past directors and the present director all came from small towns. They all showed interest in science at an early age and before graduation from high school they had in their own homes a shop or a scientific laboratory for their own use.

The book lists the improved products of the company and produces for the young reader a picture of the opportunities offered to anyone interested in science and research. He learns to know some of our most successful scientists.

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Vacuum Equipment and Techniques. A. GUTHRIE and R. K. Wakerling. Pp. 264, 16 × 24 cm. McGraw-Hill Book Company, Inc., New York, 1949. Price $2.50.

According to the advertisement on the jacket of this volume, it is concerned with the development and study of high vacuum equipment made by the personnel of the University of California Radiation Laboratory. The volume is a compilation of observations made in the course of developing high vacuum equipment for use in electromagnetic separation plants. The routine production of high vacuum in large systems on a scale never previously undertaken was required in the operation of the electromagnetic separation process; and as a consequence, the problems involved were of a magnitude which made it necessary to do a considerable amount of pioneering work on both equipment and testing. One topic of particular interest is the use of the vacuum analyzer and helium leak detector which were developed for use in the project and proved to be very sensitive instruments. It is essentially a book that has grown out of wartime activities.

When a beginner sets up a vacuum system in a laboratory, his pumps are usually the most massive parts of the equipment. The beginner usually assumes optimistically, and perhaps naively, that once he has fabricated a tight vacuum system, all that a pump has to do is to act as a valve to keep atmospheric gases from re-entering the place from which gas has diffused. In industrial practice and in a great many research projects of physics, this naive point of view must be discarded. There are always leaks of gas to be taken care of by the pumps, and in many instances gases are fed in, for example, to an ion source. These must be pumped away in order to maintain the requisite vacuum in the remainder of the equipment.

In the first chapter of the book, the necessary kinetic theory is developed in a way that is immediately applicable to the design of vacuum systems and a numerical example is worked out in some detail for a typical case in which the ion source feeds gas continually into a vacuum chamber.

The component parts of a vacuum system are treated in the chapters that follow—high vacuum pumps, booster pumps, backing pumps, cold traps, vacuum gauges, materials for vacuum chambers, seals for different materials and different purposes, leak-detecting equipment. Such a formidable array of materials, instruments, and tools is called for that a beginner might well be discouraged, if he read only this book, from ever attempting to produce a good vacuum. This attitude would be merely a reflection of the difference between a small experimental laboratory vacuum system, usually made of glass, and a production-line system in which the whole assembly has to withstand harsh treatment and full load operation almost continuously. In another respect, too, the techniques and practices described in this new book differ from those ordinarily employed by the student. In the college laboratory, little attention is paid to matching the capacity of a high vacuum pump to the system that is to be ev acuated but in a very large scale installation, power requirements have to be calculated so that there will be no unreasonable waste.

For those research students whose problems have carried them from the student laboratory stage to the larger field in which physics apparatus is built on an engineering scale, the book offers a great many helpful hints on welding of tanks, the proper disposition of gaskets, and the appropriate types of seals to be used when some motion has to be transmitted from the outside to the inside of a vacuum system.

A series of appendices summarizes the formulas that are necessary for the proper design of a pumping system, the

physical properties of some gases and vapors, the choice of appropriate oils for mechanical pumps and for diffusion pumps, the effectiveness of cold trap fillers, and of drying agents, and the properties of waxes, cements, and soldering compounds.

Every laboratory should have this book in its library, but it is not one that would be appropriate for a beginner in the art of producing high vacuum until he has emerged from the teaching laboratory stage.

The diagrams are clear, the tables and appendices are easy to read, and the material is not crowded.

THOMAS H. OSGOOD
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The Origin of the Earth. W. M. SMART. Pp. 239, Plates 8, Figs. 42. Cambridge University Press, Cambridge, England, 1951. Price $2.75.

This is no ordinary textbook; for who ever heard of a textbook on a subject about which none of the specialists —or the author—can draw definite conclusions? And yet the origin of the earth, for all its uncertainty, is a problem full of fascination for even the most elementary science student. It abounds in possibilites for the ingenious instructor who wants to cultivate in his students more thorough appreciation of scientific research than mere memorization of facts can give.

Around this intriguing question as a theme it is possible to develop major parts of the sciences of astronomy, physics, geology, and chemistry, the extent of these parts being limited only by the time available. Professor Smart, the Regius Professor of Astronomy at the University of Glasgow, draws heavily on astronomy and physics in this little book, but leaves many questions for auxiliary study in each of the four sciences. Depending on how far and in which direction such auxiliary studies are followed, *The Origin of the Earth* could be used as the core of a course extending through a quarter, semester, or full academic year, in liberal arts, physical science, astronomy, geology, or physics, either at the first-year or the intermediate level.[1]

The value of so broad a theme, particularly in elementary or liberal-arts courses, is threefold; first, it stimulates student interest by reason of its very inconclusiveness (unlike most elementary subjects in science, there is something left to argue about); it avoids the appearance of teaching elementary science by rote—like spelling or arithmetic; second, it illustrates far better than the average textbook course the interrelations between various topics within a science, and between the sciences themselves; and third, it shows how modern scientific research progresses by drawing from widely separated fields, and gives a clear picture of how these fields fit together to form a consistent picture of the physical world. Although other broad problems may also meet these criteria (e.g., the ultimate structure of matter, the extent of the universe, the origin of the elements), they generally involve more complex analysis than can be included in a first-year course.

All of this goes to show that Professor Smart has chosen an excellent topic; *The Origin of the Earth* will be enjoyed by many readers and may be used seriously, together with textbooks and original publications, by supervised students. However, it suffers from a number of serious defects: first, a "record of scientific investigations," as the preface claims this book to be, would be far more effective and would better illustrate scientific research if it carried references to original publications and to other texts, particularly in a survey as sketchy as this must necessarily be. Second, Professor Smart is sometimes out of date, both in his emphasis and in his facts, as will be shown below. Third, his style is a little ponderous, in general, for a popular audience, yet not precise enough for a serious reader. The book does not compare favorably with popular writings like Gamow's *The Birth and Death of the Sun*, on the one hand, or with H. N. Russell's more serious *The Solar System and Its Origin* published in 1935, on the other. Fourth, the organization of the material could be improved. To be effective such organization should give the reader a clear insight into the problem, rather than a mass of disconnected facts and conclusions to remember. In his preface, which bears on this point, Professor Smart states that he has "resisted temptation to include details which distract attention from the general argument." The "argument" is clear for the first two sections of the book headed "Whence?" and "When?"; but becomes obscure in the third and fourth sections headed "How?" and "Epilogue."

The four chapters devoted to "Whence?" include a conventional description of the solar system accompanied by one digression into the kinetic molecular theory of gases in connection with the study of planetary atmospheres, and by another into spectroscopy for purposes of determining the chemical elements in the sun. Both of these topics are developed rationally and with sufficient accuracy for the purpose. In fact, it would have been better if the author had followed the same rational development of his astronomical material, in the presentation of which he yields to his previous experience as writer of half a dozen conventional textbooks by including dogmatic statements and by making unexpectedly severe appeals to the reader's background. It seems anachronistic to include in the same book with an elementary description of the electromagnetic spectrum and a qualitative description of triangulation, such difficult statements as "Kepler's third law in effect states that the semimajor axis of the planet's orbit is equal to $T^{2/3}$" (p. 4), or casual references to celestial coordinates (p. 110). If these were isolated lapses in the specialist's effort to keep away from technical jargon, they would hardly be worthy of note. Unfortunately, however, Professor Smart hesitates to explain or justify the more complex conclusions of astronomy; he catalogs them as bare facts rather than demonstrable conclusions, this being in sharp contrast to his rational treatment of physics, chemistry, and geology. Evidently, he expects his readers to be astronomically mature but physically, chemically, and geologically naive.

A less dogmatic attitude on the part of the author might have softened the following statements, with which many astronomers will disagree: in connection with Mars, for

instance, he states that "the polar caps are now revealed, partly at least, as atmospheric manifestations, probably thin clouds high above the polar regions" (p. 76) and "a few of the most prominent canals have been photographed on favorable occasions" (p. 78). Most of the authorities would also disagree with his feeling that any sizable fraction of meteorites or comets come from outside the solar system (pp. 88 and 92), that there is solid hydrogen and helium on Saturn (p. 82), that the earth is cooling off (p. 62), and that "the most plausible theory to account for the formation of the moon is the resonance theory" (p. 185).

Under the second heading, "When?" the geological evidence for the age of the earth and the controversy between geologists and Lord Kelvin are well presented in the persuasive rather than the dogmatic manner. A digression on the periodic table and nuclear physics is neatly keyed into the radioactive methods of dating igneous rocks and into the sources of nuclear energy in the sun and stars. The qualitative discussion of equilibrium in stellar interiors is good, though I suspect this may be over the heads of readers who earlier needed an explanation of triangulation. One temptation the author evidently could not resist at this point, regardless of its irrelevance to the general argument, is a discussion of the atomic bomb and of the scientist's social responsibility. However, he limits this digression into social science to two and one-half pages.

The third heading, "How?" includes ample evidence that we do not yet know how the earth or solar system or galaxy or universe originated, although our studies have been able to rule out several ways in which they could *not* have orginated. Professor Smart's summary of the variety of proposed hypotheses does establish beyond a doubt that no complete theory of the origin has been developed, but its lack of organization gives the impression that nothing has been gained from these attempted theories. It would have been clearer and more profitable to treat the hypotheses in historical order, or in categories, showing what progress has been made or what alternatives eliminated. The author does specify the two categories of monistic and dualistic theories, but he fails to capitalize on this dichotomy. Historically he inverts Laplace (1795) and Kant (1755), omits Bouffon, the originator of the dualistic type of theory, and subordinates the Americans, Chamberlin and Moulton, who initiated the great activity in this field with their dualistic planetesimal hypothesis in 1898, to the English astronomers Jeans and Jeffreys, who revised and refined the dualistic idea a full score of years later. It is not mentioned that investigations by Spitzer in 1939 of the condensation—or, rather, the lack of condensation—of material pulled out of a star, cast serious doubt on all hypotheses of this nature. The author also omits Birkland, the originator (in 1917) of the electromagnetic type of theory, and devotes but a scant page and a half to the Weizsäcker theory (1945). It is generally agreed that Weizsäcker's revival of Kant's nebular hypothesis, and the modifications recently proposed by ter Haar (1950) and Kuiper (1951), now offer the greatest promise of a consistent theory.

It would of course, be an impossible task to include all suggestions ever put forward in the matter, but the omission of Haldane's "Giant Quantum" suggestion (1945), robs the reader of perhaps the most bizarre of them all.

In an "Epilogue," the author attempts to derive something definite from the mass of data and the conflicting and partial theories he has selected for treatment, but merely confuses the issue by a digression on cosmic rays. His conclusion that scientists are not infallible is one the reader will be quite willing to accept, and some may feel relieved when the author in the last page or so, retires from the field of science to the sanctuary of religion.

THORNTON PAGE
Yerkes Observatory
University of Chicago

[1] The reviewer writes from specific experience, using the problem of the origin of the earth as the central theme in one quarter of a first-year physical science course and in a more advanced astronomy course at the University of Chicago.

Introduction to the Study of Physics. WOLFGANG FINKELNBURG. Pp. 119. Carl Winter, Heidelberg.

The translated title of this little German book (*Einführung in das Studium der Physik*) may suggest another elementary physics text. In fact it is far more remarkable than that; it is an unusual introduction to those aspects of the subject, physics, which the student ought to know before he begins to study it. I have often wondered why similar books are not written for American students. Here is one that could well serve as a model for authors who wish to present to secondary school students the facts they need to make a reasonable choice of a college major.

Professor Finkelnburg's book is part of a series, called "Winter's Studienführer," which comprises several small volumes, each devoted to some special science and written by a major representative of his field. If the present specimen is a fair sample, the whole series is an enviable asset to the German scientific literature. The author's approach to the subject is well balanced. His appraisal of all issues is factual and delicate; without yielding to the sensational appeal of physics which, on this side of the ocean, is rapidly transforming parts of our science into academic circuses, he presents the subject in its proper perspective and with equal emphasis on all its parts.

The book surveys the contents as well as the large historical phases of the subject. It casts a quick glance at neighboring fields with which the physicist in his work is bound to make contact. Last but not least, it points to the psychological requirements for a successful career in the most exact of all the natural sciences. This is done against the background of the actual conditions in which physicists work, and a sketch of these conditions is included in the book. Finkelnburg describes the work of the university professor and the academic researcher; collaborators (Vieweg, Ramsauer, Lehmann, Altfeld) give interesting accounts of what the student may expect if he enters the electrical industry, the chemical industry, or the secondary school field. Mrs. Lilienthal advises the coed who wonders about her prospects in physics. Her remarks are interesting, in part amusing, but are not applicable to our milieu.

It is unfortunate that much of the excellent material in this book does not fit the American scene. In one respect, however, it carries an important message. Nobody can read it without being impressed by the degree of integration in the system of education for which it is written. The sections dealing with secondary education imply a far greater concern with that most vital stage of learning than we are accustomed to give it. Indeed physics teachers in secondary schools can draw particularly useful lessons from this *Introduction*.

Its present German edition is not likely to influence our teaching. It is to be hoped that the booklet, suitably rewritten in English, can be made available to educators in the United States.

H. MARGENAU
Yale University

The New Physics. SIR C. V. RAMAN. Pp. 141. Philosophical Library, New York, 1951. Price $3.75.

It is rare in these times that one finds a book written exclusively for the layman treating of the subject matter of modern physics. Most writings in this vein have gone into the philosophical aspects in order to avoid the mathematical rigor which the layman cannot handle. But neither can the layman handle the philosophy of physics.

It seems that Dr. Raman has avoided this quandary in a series of radio addresses which have been incorporated in this book. It is therefore not a discourse on the theoretical or philosophical aspects of the new physics as we are accustomed to thinking of writings in this field. It is rather a discussion of the new physics from an aesthetic point of view. In reading *The New Physics* I was reminded of Beethoven's *Glory of God in Nature*, since Dr. Raman continually brings out the beauty in nature which results from phenomena which in themselves are described by the physicist in mathematical terms. One will find in this short exposition discussions around subjects varying from the redness of the sunset to the crystalline structure of certain minerals, and from theories of the spectral shift of light from distant galaxies to the structure of the nucleus.

The physicist may not learn anything new in physics from this book, but he may obtain a fuller appreciation for his own work. The method of presentation, keyed to the lay radio audience in India, should prove enlightening to those especially connected with the task of presenting general physics to nonphysics majors.

Dr. Raman brings one to think not only of cyclotrons and nuclear reactors, but of the simple occurrences in nature such as soil structure, water supply, and the general subject of meteorology. Several side lights bring out points of interest which are never discussed in a scientific treatise. To mention only one, I recall his reference to the beauty, not only in structure, but in color, of the great nebula in Orion which he had the pleasure of viewing directly with the aid of the sixty-inch reflector at the Mt. Wilson observatory.

One learns more about a scientist by reading personal accounts of his life than from technical treatises he has written. This is certainly true here, for Dr. Raman devotes an entire chapter to the subject of shells, one of his hobbies. He does not leave his readers with the impression, however, that the rigorous presentation of physical principles is as simple as he has stated them when he casually drops the remark that "Physics is primarily a logical system of mathematical thought applied to the elucidation of the phenomena of nature."

THOMAS P. MERRITT
Albright College

New Members of the Association

The following persons have been made members or Junior members (*J*) of the American Association of Physics Teachers since the publication of the preceding list [Am. J. Phys. 19, 395 (1951)].

Bennett, Harold Earl (*J*), 500 Daly St., Missoula, Mont.
Blankenbaker, John Virgil (*J*), 2911 Orchard St., Corvalis, Ore.
Brown, David Wright (*J*), Washingtonville, N. Y.
Drennan, Ollin J., R.R. 1, Kirksville, Mo.
Dunbar, Lee E. (*J*), 124 Warner Ave., Hempstead, N. Y.
Glenn, Howard Bartels (*J*), 533 N. Pine Ave., Chicago 44, Ill.
Hansen, Harold John, Jr. (*J*), 905 Joslin St., S.E., Grand Rapids 7, Mich.
Hershman, J. B., Pres., Valparaiso Technical Institute, Valparaiso, Ind.
Hirshfeld, Martin A., R.F.D. 2, Newark, Del.
Howe, Robert Milton, 3439 Richard St., Pittsfield Village, Ann Arbor, Mich.
Isaman, Francis Edward (*J*), 531 Bryden Ave., Lewiston, Idaho.
Janis, Allen Ira (*J*), 1605 Chase Ave., Chicago 26, Ill.
Johnson, Arthur Hjalmer, 216 Merry St., Negaunee, Mich.
Kukowski, Jerome Francis (*J*), Rt. 11, Box 372, Tacoma, Wash.
Lago, Rudolph Michael (*J*), 1006 Freeman St., Bronx 59, N. Y.
Lowenfeld, Mortimer P. (*J*), 258 Sullivan Pl., Brooklyn, N. Y.
Marchetti, Antonio, Jr. (*J*), 138 Amherst St., Providence, R. I.
Marcotte, Lawrence, Highland, Kan.
Matschke, Arnold Merton, 612 51st St., Oakland 9, Calif.
McGrath, Harold Albert (*J*), 3485 Broadway, New York 31, N. Y.
Pippin, David Robert (*J*), Greensboro, Md.
Podolinsky, John Philip (*J*), 4241 39th Ave., So., Minneapolis 6, Minn.
Popivchak, Andrew, Jr. (*J*), 828 Rutledge Ave., Charleston, S. C.

Potosky, Maurice (*J*), 1841 Marmion Ave., New York 60, N. Y.

Sampson, Douglas Howard (*J*), Edmore, N. D.

Sowders, Ted Meredith (*J*), 520 East 60th St., Indianapolis, Ind.

Stanwick, Glenn, 2417 W. North Ave., Milwaukee 5, Wis.

Stewart, Edward William, Jr. (*J*), Cardiff, Md.

Summerfield, Patricia L. (*J*), Denton House, Houghton, Mich.

Tacheron, Robert George (*J*), 4851 Long Branch, San Diego 7, Calif.

Villena, Leonardo, Productos y Aparatos Cientificos e Industriales, S. A. Apartado de Correos 501, Madrid, Spain.

Weldon, Rodney Gorham (*J*), 2055 Fletcher Ave., South Pasadena, Calif.

Winfrey, Capt. John A., USN, 4465 Q St., N.W., Washington, D. C.

Zimmerman, Carl Lemuel (*J*), Rock Hall, Md.

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LETTERS TO THE EDITOR

Extended Bernoulli Equation

As we fully agree with James B. Kelley[1] that the Bernoulli equation is seldom deduced in a way sufficiently general to show its bearing and the restrictions of its applicability, we regret that Kelley omits to treat the most simple application of this equation in the case of steady, rotational and incompressible flow (case III), *viz.*, integration along a streamline.

In most textbooks on hydrodynamics Bernoulli's equation is derived by integrating the Eulerian equation along a streamline in the case of steady, incompressible motion. Afterwards it is shown that in the case of irrotational flow the so-called Bernoulli constant has the same value throughout the whole fluid.

In case III [steady (or unsteady), rotational, incompressible flow] Kelley states: "Now, however, $\nabla\times\mathbf{V}\neq 0$ and hence the second integral on the left-hand side of Eq. (13a)

$$[\int_c(\partial\mathbf{V}/\partial t)\cdot d\mathbf{r}+\int_c(\mathbf{V}\times d\mathbf{r})\cdot(\nabla\times\mathbf{V}) \\ =\int_c\nabla\{\Omega-P-(V^2/2)\}\cdot d\mathbf{r}]$$

remains. It is obvious that the solution can be carried out only for special cases."

However, there is a special case in which both integrals on the left-hand side of Eq. (13a) disappear, *viz.*, the case of steady, rotational flow on integration along a streamline. We feel that this case is so important that it should have been mentioned.

Combining this conclusion with another by Kelley we may state: For steady flow Bernoulli's equation is obtained when

$$\int_c(\mathbf{V}\times d\mathbf{r})\cdot(\nabla\times\mathbf{V})=0.$$

This happens: (a) In the case of irrotational flow, $\nabla\times\mathbf{V}=0$, irrespective of the path of integration ($\mathbf{V}\times d\mathbf{r}\neq 0$). (b) In the case of integration along a streamline, $\mathbf{V}\times d\mathbf{r}=0$, irrespective of the rotation in the fluid ($\nabla\times\mathbf{V}\neq 0$).

A. Klinkenberg
G. J. Sleutelberg

N. V. de Bataafsche Petroleum Maatschappij
The Hague, The Netherlands

[1] James B. Kelley, Am. J. Phys. **18**, 202 (1950).

Problems Involving Variable Mass

I WAS much interested in the recent article by Professor Chapin on analogous problems involving variable mass, moment of inertia, capacitance, and inductance.[1] However, it appears to me that there is an error in Sec. 1, which describes the rotational problem. In calculating the tangential force on the particle, we must use the Coriolis[2] acceleration $2\omega dr_1/dt$; the factor two has apparently been omitted here. With this correction, the power developed by the tangential force becomes

$$P=f_t r_1\omega=(m\times 2\omega dr_1/dt)r_1\omega=2mr_1\omega^2 dr_1/dt \\ =2mr_1\omega^2[(1/2mr_1)dI_1/dt]=\omega^2 dI_1/dt.$$

This is precisely equal to the power input to the wheel, as it should be.

For simplicity, we may neglect the wheel itself, since its energy and velocity remain constant, and consider only the sliding mass (which varies the moment of inertia I) as our system. Then the tangential force furnishes the only power *input* to this system; when we use this approach, we are merely calculating the power furnished by the belt from a different viewpoint. This power can be accounted for in three ways: (1) P_k, the increase in kinetic energy of m due to its tangential velocity, (2) P_r, the increase in kinetic energy of m due to its radial velocity, and (3) P_f, the power supplied *by* the system and dissipated *against* friction. The first term has been calculated in the article as $P_k=\frac{1}{2}\omega^2 dI_1/dt$. The second term is obviously

$$P_r=(d/dt)[\tfrac{1}{2}m(dr_1/dt)^2].$$

The last term must now be determined. The friction force must be

$$F_f=ma_r=m[(d^2r_1/dt^2)-\omega^2 r_1].$$

The power which is supplied *by* the system and dissipated *against* friction is then

$$P_f=-F_f\frac{dr_1}{dt}=-m\left(\frac{d^2r_1}{dt^2}-\omega^2 r_1\right)\frac{dr_1}{dt}=-m\frac{d^2r_1}{dt^2}\frac{dr_1}{dt}+m\omega^2 r_1\frac{dr_1}{dt}.$$

The first term may be transformed to give

$$P_f=-\frac{m}{2}\frac{d}{dt}\left[\left(\frac{dr_1}{dt}\right)^2\right]+m\omega^2r_1\frac{dr_1}{dt}$$

$$=-\frac{d}{dt}\left[\frac{m}{2}\left(\frac{dr_1}{dt}\right)^2\right]+m\omega^2r_1\frac{dr_1}{dt}.$$

The second term may be evaluated by use of Eq. (3) of Chapin's article.

$$dr_1/dt=(1/2mr_1)dI_1/dt,$$

$$P_f=-\frac{d}{dt}\left[\frac{m}{2}\left(\frac{dr_1}{dt}\right)^2\right]+m\omega^2r_1\left(\frac{1}{2mr_1}\frac{dI_1}{dt}\right)$$

$$=-\frac{d}{dt}\left[\frac{m}{2}\left(\frac{dr_1}{dt}\right)^2\right]+\frac{\omega^2}{2}\frac{dI_1}{dt}.$$

Adding these three power terms, we obtain

$$P_k+P_r+P_f=\frac{1}{2}\omega^2\frac{dI_1}{dt}+\frac{d}{dt}\left[\frac{m}{2}\left(\frac{dr_1}{dt}\right)^2\right]$$

$$+\left\{-\frac{d}{dt}\left[\frac{m}{2}\left(\frac{dr_1}{dt}\right)^2\right]+\frac{\omega^2}{2}\frac{dI_1}{dt}\right\}=\omega^2dI_1/dt=P.$$

Thus, the sum is equal to the power input, as was to be expected.

JOHN S. THOMSEN

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[1] E. K. Chapin, Am. J. Phys. **19**, 6 (1951).
[2] J. C. Slater and N. H. Frank, *Mechanics* (McGraw-Hill Book Company, Inc., New York, 1947), p. 56.

Problems Involving Variable Mass

PROFESSOR Thomsen is quite correct in his analysis[1] of the problem in Sec. 1 of my article.[2] The total tangential force is the Coriolis force $2m\omega dr/dt$. Its use leads to the total power—not its distribution in the system. The force $m\omega dr/dt$ is, however, the tangential force producing change in kinetic energy in the system. For if the tangential velocity of a particle is $v=\omega r$, its tangential acceleration is $dv/dt=\omega dr/dt$, since ω is constant. And by the second law, $f=m\omega dr/dt$.

The power to produce radial acceleration which Professor Thomsen denotes by P_r was neglected in my article on the grounds that, since the acceleration is negative, the power involved would have to be at the expense of the system. With greater generality, he shows that this term would disappear even if it were positive. We both arrive, however, at the same conclusion regarding the distribution of power supplied by the source.

ERNEST K. CHAPIN

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[1] John S. Thomsen, Am. J. Phys. **19**, 435 (1951).
[2] E. K. Chapin, Am. J. Phys. **19**, 6 (1951).

A Matter of Terminology: The Kilocalorie and the Kilomole

I WANT briefly, but loudly, to lift up my voice in protest against what seems to me to be an unfortunate and confusing terminology. Since the mks system has come into common use, the terms "kilogram mole" and "kilogram calorie" have appeared. When writing the dimensions of the quantities these appear as "kgm-mole" and "kgm-cal"; and these combinations "obviously" mean, respectively, the product of kilograms and moles and of kilograms and calories.

I suggest that, since the kilogram calorie is just 1000 gram calories, and the kilogram mole is just 1000 gram moles, the normal designations of the mks quantities would be kilocalorie and kilomole, abbreviated kcal and kmole.

FRANCIS E. THROW

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Chladni Plate Figures

IN a recent article, Miller[1] records an observation on the use of a mixture of sand and lycopodium powder in the production of Chladni plate figures. He observed that the sand gathered at the nodes, but that the lycopodium powder collected more prominently in the middle of the vibrating segments, and not along the nodal lines. The observation, the author states, appears not to have been previously reported.

Michael Faraday, however, reported precisely the same observation in 1831.[2] He proposed also the correct explanation which he later verified by performing the experiment in a vacuum. Poynting and Thomson in their textbook on *Sound* refer to Faraday's work. Their brief summary follows:[3]

> There is a curious fact with regard to Chladni's figures, first explained by Faraday. If lycopodium dust is mixed with sand on a plate, the lycopodium collects in heaps at the middle of the vibrating segments, and not at the nodal lines. Faraday showed that this was due to eddies of air. They catch up the light dust and whirl it about where they are strongest, that is, over the points of greatest vibration. When the motion ceases, the eddies die away and drop the dust immediately below the region where they existed. In a vacuum the lycopodium gathers with the sand on the nodal lines.

Arthur Taber Jones in his excellent text on *Sound*[4] gives a very complete and detailed discussion of the motion of both sand and lycopodium powder on Chladni plates. He includes a discussion of Faraday's work and also refers to earlier and later work on the same subject.

An additional statement by Miller that with sand alone or with lycopodium alone it is observed that each accumulates along the nodal lines, is not in agreement with observations by Chladni himself or with those of others.[4] Sand gathers at the nodes but a light powder, in air, gathers at the antinodes.

W. M. PIERCE

Ohio University, Athens, Ohio

[1] J. S. Miller, Am. J. Phys. **18**, 534 (1950).
[2] M. Faraday, Trans. Roy. Soc. (London) 121, 299 (1831).
[3] J. H. Poynting and J. J. Thomson, *Sound* (Charles Griffin and Company, Ltd. London, 1900), p. 131.
[4] A. T. Jones, *Sound* (D. Van Nostrand Company, Inc., New York, 1937), pp. 172–180.

On Dropping a Stone Down a Shaft

IN the construction of a railroad overpass near here the engineers are driving piles which are hollow steel tubes 75 ft long, conical in shape, 18 in. wide at one end and 9 in.

wide at the "vertex" end, essentially truncated cones. The narrow end is capped and the pile is driven this end down. To the upper end is welded a second 75-ft length of uniform cross section (18-in. diameter) and the whole pile then driven gives a tube 150 ft long, tapered in the lower half. These are finally filled with concrete to which the superstructure is fixed.

The resonance and echo phenomena in these hollow shafts I found to be remarkable. The slightest whisper or even breathing into the mouth of the shaft yielded a crystal-clear return. The result of dropping a pebble into the shaft was astonishing, and on two occasions I took two of my colleagues to the scene to witness what I wish here to report.

The stone falls freely for about 2.5 seconds (about 100 feet), noiselessly. At this point (roughly) a hissing sound is set up which increases in intensity and pitch until the stone hits the very bottom. This impact is clearly heard distinct from the whining noise. Then immediately in the wake of this report comes a very high-pitched blast which sounds like a small volume of hydrogen exploding. (Recall the elementary performance with hydrogen in freshman chemistry.) This explosive sound can be felt as it reaches the top of the shaft. The wave delivers quite a blow upon the face and the intensity of the sound is painful to the ear.

It seems clear that the falling body, compressing the air in front of it, drives a compression down the pile; this condensed packet of air is freed from its trap when the body hits the bottom, and it ascends to the top of the tubing. Where the "explosion" arises is not altogether clear.

The question arises: what, exactly, is the mechanism?

It has been suggested by an interested reader that, since the stone does not fall in a straight line or a smooth curve, it grazes the side of the pipe some distance down, or comes very close to it, and this gives rise to the hissing sound. Further, the first report is caused by the stone hitting the bottom and heard at the top by conduction through the iron pipe. The second report is caused by the sudden squeezing out of air between the stone and the cap, and comes up through the air.

It is hoped that this note will incite further inquiry.

JULIUS SUMNER MILLER

Dillard University,
New Orleans, Louisiana

An Implication of the Laplace Transformation

IN a review of Professor Thomson's recent book[1] on laplace transforms, it was noted again that a simple implication of the method had been overlooked. Since a recognition of this point is of aid to students in understanding the real translation theorems, it is worthwhile to point it out, inasmuch as a discussion of the point is not given in textbooks on the subject.

In a previous article,[2] it was pointed out that the laplace transformation formulas follow naturally from the notion of fitting a function, $f(t)$, over the interval $0<t<T$ by an approximating function, $\phi(t)$, and then allowing T to approach infinity. The approximating function appropriate to this process is

$$\phi(t)=\frac{2}{T}e^{ct}\left[\frac{a_0}{2}+\sum_{n=1}^{\infty}a_n\cos\frac{2\pi n}{T}t+\sum_{n=1}^{\infty}b_n\sin\frac{2\pi n}{T}t\right]. \tag{1}$$

We shall show that $\phi(t)=0$, when $t<0$. Let $t=T-t'$, $0<t'<T$. It follows that

$$\phi(-t')=e^{-cT}\phi(T-t'). \tag{2}$$

It is well known that the laplace transformation exists for functions of exponential order, i.e., there exist real constants M and k such that

$$|f(t)|\leq Me^{kt},\ t\rightarrow\infty. \tag{3}$$

Hence, as T approaches infinity, we have

$$|\phi(-t')|\leq \underset{T\rightarrow\infty}{Me^{(k-c)T}}e^{-kt'}=0, \tag{4}$$

since $c>k$.

It must be concluded, therefore, that laplace transforms are based upon approximating functions which must be zero for all finite negative values of the argument, and that

$$\phi(t)=f(t)u(t-a),\ a\geq 0, \tag{5}$$

where

$$u(t-a)=1,\ t>a,$$
$$u(t-a)=0,\ t<a.$$

The important point to note is that in any translation process the approximating function must not be allowed to become nonzero for negative values of the argument. Consider, for example, the problem: if $\mathfrak{L}f(t)=F(s)$, then what is the transform of $f(t-a)$? This question is trivial if it is recognized that we are really dealing with $\phi(t)$ as in Eq. (5) rather than $f(t)$ when calculating the laplace transform. Hence, we have

$$\mathfrak{L}f(t)u(t)=\int_0^\infty f(\lambda)u(\lambda)e^{-s\lambda}d\lambda=F(s). \tag{6}$$

Let $\lambda=t-a$. Then it follows that

$$\begin{aligned}\mathfrak{L}f(t)u(t)&=e^{as}\int_0^\infty f(t-a)u(t-a)e^{-st}dt \\ &=e^{as}\mathfrak{L}f(t-a)u(t-a)=F(s),\end{aligned} \tag{7}$$

which easily shows why $f(t-a)$ must be zero for $t<a$.

One additional example will suffice to illustrate the utility of dealing with $\phi(t)$, which is $f(t)$ multiplied by an appropriate unit function. Let

$$\mathfrak{L}f(t)u(t)=\int_0^\infty f(\lambda)u(\lambda)e^{-s\lambda}d\lambda=F(s), \tag{8}$$

and let $\lambda=t+a$;

$$\mathfrak{L}f(t)u(t)=e^{-as}\int_{-a}^\infty f(t+a)u(t+a)e^{-st}dt=? \tag{9}$$

Clearly, $u(t)$ is not an appropriate multiplier in this case, for the right-hand side of Eq. (9) is not a laplace transform, since $f(t+a)u(t+a)$ is nonzero with $-a<t<a$. If, however, we start with

$$\mathfrak{L}f(t)u(t-a)=\int_0^\infty f(\lambda)u(\lambda-a)e^{-s\lambda}d\lambda=F_1(s), \tag{10}$$

and set $\lambda=t+a$, we have

$$\begin{aligned}\mathfrak{L}f(t)u(t-a)&=e^{-as}\int_0^\infty f(t+a)u(t)e^{-st}dt \\ &=e^{-as}\mathfrak{L}f(t+a)u(t)=F_1(s).\end{aligned} \tag{11}$$

In conclusion then, much confusion in the minds of students can be avoided if it is realized that operations are performed on a function of the type $f(t)u(t-a)$, where a is appropriately chosen for the situation in hand.

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[1] H. M. Trent, Am. J. Phys. **19**, 391 (1951).
[2] H. M. Trent, Am. J. Phys. **17**, 507–510 (1949).

Some Laboratory Tests

PROFESSOR J. S. Miller has described a type of laboratory examination[1] in which an essential piece of equipment is omitted from the collection given to the student at the beginning of a laboratory period. The same sort of additional experience can, I believe, be provided in a simpler way by having students find errors that have been intentionally included in setups with which they are already familiar. A few examples are given.

In a high school laboratory, it is important to instill in the student the love of truth, and honesty of observation becomes an important factor. The first two experiments are designed with this in mind.

1. In the experiment on the inclined plane, I use a board' one side of which is waxed and polished and the other side dull. The problem on which the student is tested is "Calculate the efficiency of the inclined plane." From previous experience students usually assume that the waxed polished surface should be more efficient, whereas the board is so made that the reverse is true. Some of the boys will even doctor up their results to conform with their preconceived notions. It is only by doing the experiment that students learn the nature of this particular inclined plane, and to record honestly their observations.

2. Another experiment which might serve to test scientific attitudes is to hand out lenses to a class and ask them to get an image on the wall, and record the image in their notebooks. If half the lenses are convex with a focal length of from 6 to 10 inches, and the other half concave, the student with one of the latter lenses should be willing to record "no image," while some of the other students are getting images. Honesty in observation is essential at the high school level.

3. In reviewing series circuits, I usually connect a 60-watt lamp and a 7-watt neon lamp in series to a 110-volt circuit. The neon lamp will light, but the 60-watt lamp will not. "Is there any current in the tungsten lamp?" is the question.

4. A dead storage battery can be doctored up to give a 1.3 reading on a hydrometer, and fair no-load voltage reading, but still not work in an experiment. The students are asked to account for the discrepancy between the theoretical and the practical behavior of the battery. It is only when a thoroughly trained science student takes a voltage reading under load that he discovers the dead battery.

5. A rather spectacular experiment which must be performed carefully and individually, is to hand a student a dry cell and a galvanometer, internal resistance 20 ohms, and ask him to find out whether the dry cell is good. Practically any student in the class can test a dry cell with a voltmeter, but how many students in a class will check the type of meter they are using before checking the cell? The student should be stopped before connecting up the circuit, and told what will happen when a 1.5 volt dry cell is shorted across a 20-ohm galvanometer coil. This can be shown rather spectacularly by shorting a Nichrome wire across a power source, and burning it up.

6. An electronics unit offers the best type of laboratory examination when the phase on trouble shooting is started. The students work in small groups, and each group is given a radio set in playing condition. The instructor then puts one error in the set, and the student is to find out why the set does not play. For example, by the simple process of interchanging 2 tubes, the set can be rendered inoperative. Some students spend the entire two-hour period without finding the trouble.

Another error which may be put into a set is the shorting of the plate-load resistor with a very thin, carefully concealed wire. Or, once the student has found this trouble, he can be confronted with an open resistor in the plate circuit. However, the instructor must be careful that the resistor is identical with the original, and in no way should the student be able to find the trouble by mere observation. Removing the cathode bias, for instance, is not a good problem, because the student can usually notice something missing by mere observation. A somewhat simple problem is to remove a connection on the input to the radio so that no power goes to the set.

I found that, in general, a laboratory test is considered more enjoyable to take than either a written test or an oral one, and at the same time the students learn well.

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[1] J. S. Miller, Am. J. Phys. **19**, 191 (1951).

Causality, Relativity, and Language

IN a recent article[1] on causality, relativity, and language, it is suggested that language fails but mathematics succeeds in describing physical phenomena, the reason being that the words of everyday experience are poorly adapted to describe some of the unusual situations encountered by the physicist. It is true that everyday words may fail, but the physicist devises a technical vocabulary to describe unusual situations and thus gets around the difficulty.

The word discussed in the article, causality, is from the lay rather than the technical vocabulary; and the definition given of causality seems to be nothing more than an expression of the old fallacy: after this, therefore on account of this.

Further analysis of the relativity of the time order of events leads to some interesting conclusions. Consider two

inertial frames of reference, initially in coincidence, with the origin of one moving with uniform velocity v along the positive x axis of the other. If, in the latter, an event occurs at x_1, at time t_1, and a second event occurs at x_2 at time t_2, the corresponding time interval in the former system is[2]

$$t_2'-t_1'=\frac{t_2-t_1-v/c^2(x_2-x_1)}{(1-v^2/c^2)^{\frac{1}{2}}}, \tag{1}$$

where c is the velocity of light.

First consider $t_2>t_1$. The greatest velocity with which an agency connecting cause and effect can be propagated is c. Hence, if there is a causal connection between the events at x_1 and x_2, it follows that

$$t_2-t_1 \geqslant (x_2-x_1)/c \geqslant 0. \tag{2}$$

Since v/c must be less than 1 for $t_2'-t_1'$ to be real, it follows that $t_2'>t_1'$ irrespective of the sign of v. Hence, the time order of two causally connected events cannot be reversed.[2]

Furthermore, in the limit, as t_2 approaches t_1, x_2 approaches x_1, and therefore t_2' approached t_1'. Hence, although simultaneity is relative for events not causally connected, two causally connected events are either simultaneous or not simultaneous in all inertial frames of reference.

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[1] Elihu Fein, Am. J. Phys. 19, 211 (1951).
[2] G. Joos, *Theoretical Physics* (G. E. Stechert and Company, New York, 1934), p. 231.

Causality, Relativity, and Language

THE conclusions that the time order of two events causally connected for an observer in the same inertial system cannot be reversed by any other observer and that two causally connected events which are not simultaneous for an observer in the same inertial system are not simultaneous for all observers are, of course, true. But the question which was raised by Fein was: May two events which are temporally separated for a given observer be interpreted by him to be causally related? The fact that these two events may appear to be simultaneous for an observer at rest with respect to these events cannot alter his interpretation as long as he seeks to translate the mathematical or symbolic description of these events into causal statements (that is, into the verbal language).

In a word, causality is a space-and-time concept, not a space-time concept. It is, figuratively speaking, not invariant in the relativistic syntax. This is a handicap of the verbal language as the article indicates.

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Slugging Out a Case for the Pounders

IN an interesting article with the above title,[1] S. L. Gerhard recommends that the pound be adopted as a unit for both mass and force and that the dimensions of mechanics be expressed in terms of four fundamental quantities, mass, force, length, and time, instead of force, length, and time; or mass, length, and time. The primary argument advanced in favor of this recommendation is that in actual practice the ordinary person does not make any distinction between the unit of mass and the unit of force and that these two units have become the same in practice because of convenience. Mr. Gerhard cites an example stating that the amount of nourishment in five pounds of sugar is considered by most users of sugar to be proportional to the mass although the effort in carrying this sugar is recognized as dependent on the weight. The common confusion associated with matters of weight and mass are illustrated by his sentence, "But once in the kitchen you can easily determine whether you received honest weight (mass) by weighing it." This sentence illustrates the fact that the nonscientific person, in purchasing sugar or any other material, thinks not in terms of mass but in terms of weight. He considers the nourishment in sugar or the heat equivalent in fuel as proportional to the weight and not proportional to the mass. This can be easily verified by asking anyone, even college graduates and physics majors, their understanding of the word mass. A few may make the remark that mass is the quantity of matter, but if then you inquire further as to what is meant by quantity of matter, there will be found little understanding of this concept. It is stated by Mr. Gerhard that quantity of matter is one of the first ideas we learn in childhood. This seems very questionable, since in learning the properties of matter by playing with objects, a child can gain a concept of length and area and volume and weight and may gain a vague concept of inertia by applying a force to the object and observing the resultant acceleration. Direct observations made by a child do not include something which could be denoted quantity of matter or mass. The phrase "quantity of matter" is used in perhaps half of the textbooks as a definition of mass and yet this phrase has no definite meaning. Quantity of matter cannot be defined and it cannot be recognized as one of the undefinable properties of matter which we experience directly through our senses. There is no sense for the direct perception of mass. There is, however, a very definite sense for the direct perception of force. One should not, therefore, place mass and force on the same basis as fundamental undefined physical quantities.

It should probably be recognized by teachers of physics and engineering that the concept of mass is a scientific concept which is utilized primarily only by scientists. As such, this concept should be logically related to fundamental concepts in a manner which admits of no confusion to either scientists or others. The practice of nonscientists should be recognized to be a practice of description in terms of weights rather than in terms of mass.

To clarify and relate the concepts of physics it is necessary that each physical quantity shall either be specified by an exact defining equation or specified as an undefinable physical quantity perceived directly through the senses.[2] Force is a quantity which can be perceived directly through the senses and can therefore be specified as an undefinable

physical quantity, but mass is not directly perceptible by the senses and must be considered as a defined physical quantity. Newton's second law, force/acceleration = constant, can serve as a definition of mass; and it is preferable to let this ratio be only one constant rather than the product of two constants.

If mass is defined by the equation, $m=F/a$, the definition applies under all conditions as long as the user confines his units to one system. There is no necessity for insertion of the factor g and therefore no possibility of forgetting when to insert g. In case a problem should state the weight of an object when the equation to be used contains the mass, the student must first determine the mass from the relation, $m=W/g$. This is not the definition of mass but is the equation relating mass and weight. If W is in pounds, the mass is obtained in slugs. There is no uncertainty as to when to use g and when not to use g. The factor g is never used in the equation $F=ma$.

The desirable aim for scientists is to provide a method of describing properties of matter which is logically consistent and as free from confusion as possible. Such logical consistency requires that only a single system of units be used at one time. If in this system of units we have force measured in pounds, it is necessary to have another unit for the measurement of mass. This unit is the slug. When, however, mass is measured in pounds, it should be recognized that a different system of units has been adopted; and in this new system of units force must be measured in poundals. Thus, in any one system mass and force are not measured in the same units, and the definition of mass is the same regardless of the system of units. In conclusion it can be said that although Mr. Gerhard has found his system to be teachable to his own students, this is something that can be said of any system which is well understood and liked by the teacher. That can be said also of the system which is here recommended for the use of the slug and the corresponding unit in the metric system, sometimes called the gravitational gram. Mr. Gerhard has provided a very interesting discussion in his "Slugging out a case for the pounders," but it still seems to be not as strong as the case which can be pounded out for the sluggers.

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[1] S. L. Gerhard, Am. J. Phys. **18**, 302 (1950).
[2] J. G. Winans, Am. J. Phys. **3**, 142 (1949).

Slugging Out a Case for the Pounders—A Reply

IN my original article[1] I endeavored to show that the pound as the unit of mass is in common usage, and that the slug is rarely ever employed, even in engineering, where it was first suggested. I further showed that the equations $\mathbf{F}=m\mathbf{a}/g$ and $m=w$ enable us to incorporate the pound mass unit into our kinetics equations.

The most obvious argument in favor of the pound unit of mass is its popularity, as reflected in the handbooks of various technological fields, and by its use by practicing engineers. Not one of the current handbooks lists the slug as a unit of mass. In the majority of them it is not even mentioned. In one, which includes sections on aeronautics, the comment is made that "The slug (also called the 'geepound' or the 'engineer's unit of mass'), the 'metric slug' [Mr. Winans' gravitational gram], and the 'poundal' are rarely ever used in practice"; and on the same page is the statement: "The word 'pound' is used for the unit of both force and mass. . . ."[2] The Oxford Dictionary (1919) does not mention, among a dozen rare usages of the word, the use of "slug" as a unit of mass, even though it is quoted in some textbooks as "the British engineer's unit of mass." A random canvass of engineers among my acquaintances revealed that the pound is their preference, and that they use the slug only when forced to by convention in certain fields, largely in aeronautics. Those engineers who are not teaching and are not in aeronautics seem to have forgotten the meaning of the word.

In discussing mass *versus* weight concepts, Mr. Winans' first point is that the layman thinks in terms of weight and not mass. On several occasions I have discussed this question with laymen and students, in order to discover what they really think. For instance, among cooks the amount of food is estimated by its volume or bulk; for example, one pound of sugar is visualized as 2 cupsful, etc. Further questioning about whether the same pound of food would yield the same nourishment when taken to another planet, where it might *weigh* less, brought affirmative replies, indicating that their concept of "pound" is the mass concept, and not the weight concept. In the common man's terminology the wrong word is used for mass, but by appropriate questioning it is readily shown that he means mass. This meaning of the word "pound" is based upon usages adopted by no less authorities than Thompson and Tait[3]—"According to the common system followed in modern mathematical treatises on dynamics the unit of mass is g times the mass of the standard or unit weight. This definition, giving a varying and a very unnatural unit of mass, is exceedingly inconvenient. In reality, standards of weight are *masses*, not *forces*. It is better, though less usual, to call them *standard masses* than standard weights, as weight properly means force. . . . They are employed primarily in commerce for measuring out a definite *quantity* of matter; not an amount of matter which shall be attracted by the earth with a given force."

In a more recent book Franklin and McNutt say—"In buying sugar or coal by the pound the word pound is used in its legitimate sense as a unit of mass."[4] Since when has this meaning of "pound" become illegitimate or obsolete? The facts of the case seem to be contrary to Mr. Winans' conclusion. It should thus be recognized by physics teachers that the concept of mass is employed by laymen as well as by scientists, even though the former erroneously call it weight. The term mass should be introduced into the layman's vocabulary to supplant weight wherever proper.

The phrase, ". . . proportional to the weight and not proportional to the mass," seems somewhat illogical. As weight and mass are always proportional to each other (in nonrelativistic physics), the nourishment in food will always be proportional to both, but with different constants of proportionality.

In the discussion of force *versus* mass units there is a reference to ". . . a very definite sense for the perception of force," and later on the statement that "Force . . . can be perceived directly through the senses. . . ." If the sense for force perception is so very definite, it would be logical, and also very illuminating, to name it outright, and not refer to a "very definite sense" in one phrase, and to "the senses" in another.

As Mr. Winans properly claims, the aim of scientists should be to provide a logically consistent method of describing the properties of matter. In this spirit let us examine some of the presently accepted logical procedures. The concepts of mass and force are admittedly different, and may have different functions in the logical structure of physics. The choice of *units* for measuring them was a natural step in the development of science, but the perceptibility (or imperceptibility) of force and mass is irrelevant to this choice of units, simply because our senses are too inaccurate to serve as measuring devices. If our senses can perceive only force and not mass, then an arbitrary unit of force, such as the "grunt," should have been adopted, independent of other concepts. But this procedure was not followed; instead, a unit of mass was first arbitrarily chosen, and then the unit of force was defined as the weight of this mass, with no reference to our "force sense." Thus the definition of the *unit* of force always devolves upon a previously chosen arbitrary unit of mass. "In any system of units, force is a quantity of dimensions MLT^{-2}."[5]

In many textbooks the definition of the pound mass unit comes first, as an object kept as a standard. The definition of the pound force unit follows as the weight of this standard mass. Several pages or chapters later, after the unsuspecting reader has forgotten its lowly origin, the unit of force is now called the "fundamental" unit, and a new unit of mass, the slug, is introduced as a "derived" unit.[6] This silly circumlocution is accepted as logical. We do not define a unit of force until after we have chosen an arbitrary unit of mass; then and only on this basis do we define a unit of force. (Whether we *could* define a unit of force without the intervention of a unit of mass is another question; we have not yet done so.) In the conventional procedure the use of Newton's second law to define a "kinetic" unit of mass is superfluous, because we have already used a unit of mass to define the unit of force in the first place. The arbitrary unit for that evasive, sensibly imperceptible, and meaningless thing called "quantity of matter," is still the cornerstone of all our systems of force and energy measurements.

The slug is merely a multiple of the pound, like the ton, hundredweight, and stone. The only excuse for its use is to make $k=1$ in the equation $F/a=km$. This is convenient for the small minority of theorists who restrict their activities to algebraic calculations, but it is most inconvenient for the many who make numerical calculations involving the units used in daily life. There is a strange conflict of loyalties somewhere in this confusion. The slug was originally an engineer's unit, not particularly welcomed by physicists, who immediately recognised the undesirability of having two separate mass units. The slug has had fifty years in which to prove its usefulness and popularity, and it has failed. It is thus peculiar that any physicist should champion an outmoded engineer's unit.

In considering the units to be used in various systems it must be remembered that units are independent of systems and equations. We are free to employ the standard pound mass in the practical gravitational system, as well as in the absolute system, in the same way that the much-touted mks system uses the newton as the unit of force in both the rationalized and unrationalized electrical systems of units. Any unit can be used in any system by proper choice of proportionality constants. Why this flexibility cannot be exercised in mechanics is a mystery. The choice between pound and slug involves more than mere logic; it depends also upon the convenience to the greatest number of people.

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[1] S. L. Gerhard, Am. J. Phys. **18**, 302 (1950). The choice of the pound or slug is not influenced by the variation of g over the surface of the earth, as intimated by some commentators since the original article appeared. The effects of this variation have been thoroughly discussed by W. W. Sleator, Am. J. Phys. **15**, 251 (1947), a reference already given.

[2] L. S. Marks, *Mechanical Engineers' Handbook* (McGraw-Hill Book Company, Inc., New York, 1951), fifth edition, p. 73.

[3] W. T. Thomson and P. G. Tait, *Elements of Natural Philosophy* (Oxford University Press, London, 1873), p. 56.

[4] W. S. Franklin and Barry McNutt, *Mechanics and Heat* (McGraw-Hill Book Company, Inc., New York, 1910), p. 84.

[5] A. E. H. Love, *Theoretical Mechanics* (Cambridge University Press, London, 1921), p. 39.

[6] Hausmann and Slack, *Physics* (D. Van Nostrand Company, Inc., New York, 1939), second edition, p. 13 and p. 71. F. W. Sears and M. W. Zemansky, *College Physics*, (Addison-Wesley Press, Cambridge, Massachusetts, 1948), p. 2 and p. 69.

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RECENT MEETINGS

Chesapeake Section

The annual spring meeting of the Chesapeake Section of the American Association of Physics Teachers was held at the University of Delaware, Newark, Delaware, on Saturday, April 14, 1951. About seventy members and friends from Delaware, Pennsylvania, Maryland, District of Columbia, and Virginia were present.

The new officers for 1951–52 are: President, G. E. C. KAUFFMAN, *University of Delaware;* Secretary-Treasurer, J. H. MCMILLEN, *University of Maryland and U. S. Naval Ordnance Laboratory;* Member of the National Executive Committee, E. R. PINKSTON, *U. S. Naval Academy;* Member of the Executive Committee, CLARE DRISCOLL, *Capitol Page School, Washington, D. C.;* Member of the Executive Committee, R. T. COX, *Johns Hopkins University.*

The program consisted of three invited and nine contributed papers.

Invited Papers

The relegation of ice. M. W. ZEMANSKY, *The City College, New York*.—The familiar lecture-room demonstration of a weighted metal wire cutting through a block of ice was treated historically, mathematically, and experimentally. A theory appropriate to a metal ribbon of rectangular cross section was developed along the lines laid down by Ornstein in 1906 for a cylindrical wire. The rate of motion through the ice was calculated in two ways: (1) by considering the heat conducted across the metal and the water film under the metal and (2) by considering the viscous flow of water from the bottom ot the top of the metal. Comparison of these two expressions with experiment led to the conclusion that the average viscosity of material transported from below to above was about one thousand times that of pure water. An approximate equation was developed for the equilibrium curve made by that part of the wire imbedded in the ice. A suggestion for future experimental work on metal spheres rather than wires was made.

Teaching atomic physics to engineers and other insolvable teaching problems. C. W. UFFORD, *University of Pennsylvania*.—The relation of atomic physics to the other mathematics and physics courses at the University of Pennsylvania was considered. There, atomic physics is the third semester of a one-and-a-half-year course in introductory physics for engineers. Among the specific items discussed were contents of the course, the extent to which relativity and quantum mechanics should be introduced, the importance of background and derivations to an understanding of the material presented, the function of memory, contrasting methods of approach from the outside of the atom inward or from the inside outward, laboratory experiments (no longer done with weights and meter bars), the difficulties students face in appreciating the delicate character of the apparatus, and the development of a sophisticated point of view.

Putting the physics in the teaching of astrophysics. L. C. GREEN, *Strawbridge Observatory, Haverford College*.—To interpret the spectra of the sun and stars, it is necessary to understand something of atomic and nuclear physics. Students taking astrophysics courses are usually not sufficiently advanced in their undergraduate work to have encountered the particular aspects of physics which are most useful. A course to meet this need has been designed. It consists of an elementary introduction to the quantum mechanics of atomic spectra and quantum statistics with numerous physical and astrophysical illustrations. The necessary mathematical tools are developed as needed. This course was described in some detail.

Contributed Papers

***B-H* curves with a lecture table magnetometer.** T. B. BROWN, *George Washington University*.—A very simple magnetometer arrangement was described which permits the plotting of *B-H* curves, point by point, with a spot of light on the blackboard.

Coulomb friction in the motion of two blocks. J. S. THOMSEN, *University of Maryland*.

A microwave analog of the optical quarter-wave and half-wave plates. G. BIRNBAUM, *National Bureau of Standards*.—The paper dealt with the theory and construction of a lattice metallic cylindrical rods smaller in length than half the wavelength. Experiments at a wavelength of 10 cm and 3 cm dealing with the use of such a lattice to produce circularly polarized and elliptically polarized waves were described.

Physics of the Chesapeake section. C. H. VOELKER, *Johns Hopkins University*.—An interim report was given on a geophysical study of estuarology which attempts to adapt some theories of physical oceanography to the shallow waters of the Bay and tidal rivers of the Chesapeake section. The paper discussed specifically the problems of predicting the density of the water in the Bay above the Potomac, given for consideration hydraulic data from the Susquehanna River basin, but omitting direct meteorological data on precipitation, wind stress, and oscillatory tidal forces. It was reported that the mean monthly flow of the Susquehanna was in no way related to the mean monthly salinity in the Bay. However, it seemed necessary to assume that the fresh water mixed with the ocean water and that this mixed water's density would be a function of the fresh water influx. As a first approximation, it was shown that about a year of water data for Susquehanna flow, when mixed analytically, did describe the density of the Bay water at a selected location. Using S gm/kg for salinity as a term descriptive of relative density, and F in 10^2 ft^3/sec for the weighted mixed flow data, the following empirical relation obtained $F = 64 - 0.25S - 0.125S^2$.

An elliptic mirror for lecture demonstration. J. SMITHSON AND W. T. FENHAGEN, *U. S. Naval Academy*.

An interesting type of problem in physics. R. R. MEIJER, *George Washington University*.—In an adavnced undergraduate course in atomic physics at the George Washington University, an endeavor is made to discuss in detail the experimental as well as the theoretical aspects, because there is no concurrent laboratory. To supplement experimental discussion, students are asked to solve problems of the following sort: "Devise an (original if possible) experiment by which a student in this course could measure the value of *e*. Give details of apparatus and procedure. For suggestions, read the reference books." Numerous other quantities are substituted for *e*. The student reaction has been interesting. Many get acquainted with reference books for the first time. Some show considerable ingenuity in devising experiments as the discussed exampled showed. All in all, the students seem to gain some appreciation of experimental physics. This type of problem is not new. One problem of this sort is in Semat's *Atomic Physics*. Professor Koehl tells me that he has been doing a similar thing in the general physics laboratory at George Washington University. This indicates that this type of problem has wide applicability.

Ten-channel time sequential analyzer. F. C. WHITMORE, P. R. LILLER, AND H. FEENY, *University of Delaware*.—The problem of the analysis of the number and time relationships of spurious counts in a self-quenched Geiger-Müller counter has been of interest for several years. Many methods have geen proposed and used for this work, but the method of interval selection seems to offer the most direct approach. The equipment presented was the result of attempts to remove the difficulties of the previous circuits. A pulsed x-ray source served as the ionizing radiation to actuate the counter under test. The analyzer detected time coincidences between spurious counts that were generated by the counter and timing gates that were generated by the delay line. The real count, resulting from the passage of the x-ray photons through the counter, actuated the delay line and thus assured an absolute time base. The circuit was so arranged that the only counts recorded were due to spurious counts. The data taken thus far were inconclusive on this point so no report of conclusions was made. A complete description of the circuit and an analysis of the results obtained was to be published in the near future.

Training in undergraduate physics. C. H. VOELKER, *Washington College*.—A recent survey has expressed the majority opinion that physics departments should not rely on mathematics departments, but should themselves add substantial work in mathematical physics at the undergraduate level to combat the elementary character of much of college physics. Washington College has added a four-hour course, actually a sequence in mathematical and theoretical physics based on such texts as Joos, Page, and Houston. The problem was then how to lead the students up to the level of this classical general physics from the advanced point of view. Washington College students use the book by Margenau, Watson, and Montgomery, thus employing calculus in the first course. They follow up with Loeb, *Electricity and Magnetism;* Jenkins and White, *Optics;* Richtmyer and Kennard, *Modern Physics* (using Harnwell and Livingood or Hoag and Korff for a laboratory manual). We would be interested in suggestions on which mechanics and which heat textbooks would be appropriate in such a series. The department also offers work in biophysics, applied electricity, applied electronics, meteorology, astronomy, and chemical physics. It was too early to describe tangible results, but it has been pleasant to see a student leaf through a common verbal physics text, and noting the conspicuous absence of analytical methods of mathematical exposition, remark, "What is this, some kind of arts' text?" We should like to hear opinions on whether or not we should extend our theoretical classical physics and teach theoretical quantum mechanics, or is this still to be the first graduate school course?

Physics in the Civil Defense Program. G. E. C. KAUFFMAN, *University of Delaware*.—In these days of national crisis the Civil Defense Program is moving ahead rapidly. A very important topic in this program is Atomic Energy Indoctrination, which is largely applied physics. The physics department is able to fit nicely into this program and it will tend to let the public know that at times we come out of our laboratories and classrooms, and are eager to help in community programs. We, in physics, know about the atom, nuclear forces, radia-

tion, and related topics; but in our local communities many do not know anything about them, and in many cases erroneous ideas are maintained which in the time of a disaster would spell chaos. If we do not get into this program and assist, much will be lost that could further our cause and interest people, especially those in high school, in our subject. This has been done by the speaker throughout the state and we believe is going to help our situation in these times of decreasing enrollment.

J. H. McMillen, *Secretary*

Southeastern Section, American Physical Society

The seventeenth Annual Meeting of the Southeastern Section of the American Physical Society was held at the University of Chattanooga, Chattanooga, Tennessee, on April 5–7, 1951, and was attended by approximately three hundred members and guests. A splendid program of sixty-five contributed papers and five invited papers was arranged by a committee of which Dr. A. H. Nielsen of the University of Tennessee was chairman. The abstracts of the nine contributed papers of interest primarily to teachers appear below and those remaining will be published in the *Physical Review*. At an evening session, Professor Eric Rogers, *Princeton University*, presented a demonstration lecture on Surface Tension. The dinner speaker was Dr. F. G. Slack of *Vanderbilt University*, whose subject was Physics and the Emergency. The other invited papers were:

An experimental course in reactor physics at the Oak Ridge School of Reactor Technology. E. C. Campbell, *Oak Ridge National Laboratory.*

Neutrons as waves and particles. C. G. Shull, *Oak Ridge National Laboratory.*

Structure and physical properties of the interstellar gas clouds. Bengt Stromgren, *Yerkes Observatory.*

The many details of arrangement of the meeting, including entertainment and visits to some of Chattanooga's industries, were admirably cared for by a committee having Professor M. S. McCay of Chattanooga as its chairman. An extensive exhibit from the American Musuem of Atomic Energy, a part of the Oak Ridge Institute of Nuclear Studies, attracted many visitors.

At its meeting the Section elected W. G. Pollard, Executive Director of the *Oak Ridge Institute of Nuclear Studies*, as Chairman for 1951–52. Other officers elected were Walter Gordy, *Duke University*, Vice-Chairman; Dixon Callihan, *Oak Ridge National Laboratory*, Secretary; R. T. Layemann, *Emory University*, Treasurer; J. H. Coulliette, *University of Chattanooga*, member of the Executive Committee. North Carolina State College at Raleigh was chosen as the site of the 1952 meeting. Eric Rodgers, University of Alabama, was the retiring chairman.

Contributed Papers

1. A laboratory course in modern physics. Carl C. Sartain, *University of Alabama.*—Our course in general physics is followed by a three semester-hour course in modern physics. The accompanying laboratory, which may be elected for an additional hour's credit, is designed to convey to the student as much knowledge of modern physical experimentation as is practical at this level along with an appreciation of the values assigned to the many physical constants discussed in class. Simple experimental methods of measuring c, e, e/m, R, N, k, λ, h, etc., are discussed; and references to detailed instructions for each experiment are presented. The problem of setting up the program and adequately equipping the laboratory is partly overcome by sharing equipment with other laboratories, particularly the advanced laboratories in electricity, light, and electronics.

2. Design and performance of classroom and laboratory wind tunnels. J. C. Herman, B. V. Rhodes,* and M. S. McCay, *University of Chattanooga.*—An illustrated discussion of the design of the University of Chattanooga's eight-foot wind tunnel built for testing various standard and nonconventional airfoils. Demonstration of various lecture room experiments in aerodynamics with a small replica tunnel, flow meters, pressure gauges, and other accessories.

* Now at the Chance-Vought Aircraft Company, Dallas, Texas.

3. Lecture demonstration of coupled systems employing selsyn motors to provide variable coupling. Lloyd W. Morris, *Louisiana State University.*—Two massive pendulums are swung from the shafts of two selsyn motors mounted upon a rigid support. The selsyns are electrically connected and the coupling between the pendulums may be varied upward from zero by exciting the selsyns with a variable 60 cps voltage from a Variac. The masses of the pendulum bobs may be altered to show the effects of varying ratios of energy storage in the two systems. The lengths of the pendulums are adjustable to exhibit amplitude and phase variations in the neighborhood of resonance. The normal modes are easily demonstrated, as well as transient and steady-state phenomena.

4. An apparatus for the study of centrifugal force and rotational inertia for use in general physics laboratory. W. L. Kennon, *University of Mississippi.*—An iron disk (10 by $\frac{1}{2}$ inch) is mounted horizontally on a central shaft and pivoted on bearings. A smaller wood disk is attached concentrically beneath the iron disk. A fishline carrying a weight is wound on the wood disk and is run over a small pulley so as to have a vertical fall of about one meter. The falling weight sets the disk in rotation. Upon the upper surface of the iron disk a brass ball is mounted at the end of a restraining spring. At a certain rotation speed the ball closes an electrical contact and flashes a light. The distance of fall of the driving weight is adjusted so that the weight is released when the light flashes. The linear speed of the ball is calculated from the distance and observed time of all of the driving weight. The radius of the path of the ball is measured. The centrifugal force is then calculated in the usual way. Also, the rotational inertia of the system can be calculated from the same data and compared with the value obtained from the mass and dimensions of the rotating system.

5. A third-semester physics course. J. Gordon Stipe, Jr., and Isabel Boggs, *Randolph-Macon Woman's College.*—This paper describes a three-hour one-semester course that follows our six-hour introductory course. It has as a prerequisite one year of college mathematics, including trigonometry, analytical geometry, and elementary calculus. The course has been given for two years and has proved to be very successful. The course is based on experimental work, with experiments selected to illustrate different experimental approaches to problems and several methods of analyzing data. The objective of the course is an understanding of the points of view, attitudes, and methods of physics; and topics treated are selected to meet this objective without regard to subject matter. A brief description of the experiments used will be given, together with reasons for selecting these particular experiments for the course.

6. Use of recording-controlling instruments in the intermediate heat laboratory. Joseph W. Straley, *University of North Carolina.*—Many experiments conducted in a heat laboratory require observation of rate processes and consequently require many hours of data collecting. Such experiments are rendered quite simple by automatic recording or recording-controlling apparatus. While there are several other fine instruments available commercially, the author's experience with an L & N Micromax has shown it to be quite satisfactory for a large variety of experiments in which the quantity under observation does not change rapidly with time. The following have been performed satisfactorily with this instrument: (1) calibration of a thermocouple, (2) study of phase diagram of metal mixtures, (3) measurement of diffusivity by periodic heating, (4) measurement of emissivity and radiation constant, (5) measurement of specific heat of iron as a function of

temperature, (6) measurement of latent heat of fusion of metals, and (7) study of natural convection in gases.

7. Introduction of physics to freshmen. N. GOLDOWSKI, *Black Mountain College, Black Mountain, N. C.*—A course for freshmen entitled "Introduction to Science" is proposed. This course has a two-fold purpose: (1) to improve the mathematical "language" of the student, (2) to develop in him the habit of using this "language" for "translation" of physical situations into mathematical symbolism. To achieve this capacity of "translation," mathematics and physics have to be taught by the same teacher, who chooses the consecutive topics of physics according to the mathematical progress of the student. Thus, the ideas of function, variable, and limit, necessary for the understanding of mathematics, can be given a physical meaning in any and all parts of the physics curriculum. Linear equations find an excellent demonstration in the study of the effects of heat; vectors are illustrated by the study of forces; trigonometric functions, *per se*, supplement the notion of vectors and allow the study of mechanics and geometrical optics; the periodicity of trigonometric functions illustrates physical optics, etc. Thus, at the risk of being presented with an incomplete physics curriculum, the student gains in understanding of physics and automatically is introduced to logical thinking, preparing him for the study of any subject.

8. A multiple frequency standard employing a modulated television-type raster for comparison of frequencies. THOMAS J. YEADON AND LLOYD W. MORRIS, *Louisiana State University.*—The construction, operation, and application of a multiple frequency standard is described. This standard can be compared readily with WWV, and provides a means by which a very large number of frequencies can be established with accuracy. The system employs a series of linear sweep generators, each synchronized at a submultiple ratio of the previous generator; the whole array being stabilized by a hundred kilocycle crystal-controlled oscillator. The sweep generators may be monitored constantly, as a means of checking their stability in relation to each other and to the hundred kilocycle standard during operation. By applying any two of the sweep voltages to the horizontal and vertical deflecting plates of a cathode-ray oscilloscope, a television-type raster is produced on the screen. The raster may be modulated by a periodically varying voltage applied to the x, y, or z axis of the oscilloscope. A characteristic stationary pattern is produced for each value of modulating frequency equal to the sum or difference of integral multiples of the horizontal and vertical sweep frequencies. The method of presentation suppresses background noise and is especially applicable to the measurement of frequency of complex wave forms.

9. A laboratory experiment on radial heat flow. R. E. SELLERS AND E. SCOTT BARR, *University of Alabama.*—Apparatus has been designed for an experiment on the determination of thermal conductivity of poor conductors, using radial flow. It is simple both in construction and use, and sufficiently accurate for intermediate laboratory work. The device consists essentially of a rubber (or glass) tube one meter long, supported vertically in a concentric steam jacket. Water from a constant temperature tank flows through the tube, and is collected in a graduated cylinder. When the steady state has been attained, the outlet temperature is measured with a mercury thermometer, and the rise in water temperature is determined by use of a differential thermocouple. The conductivity is then computed by use of the usual radial flow relation. Desirable features of the arrangement are (1) no need for corrections for radiation losses, (2) minimum distortion of the tube, (3) easy identification of the steady state. The error arising from neglect of the gradient through the stagnant water film on the tube wall can be studied by use of tubes with the same bore, but different wall thicknesses. The variation in temperature difference through the tube wall from top to bottom is sufficiently small in practice to have little effect on results.

DIXON CALLIHAN, *Secretary*
